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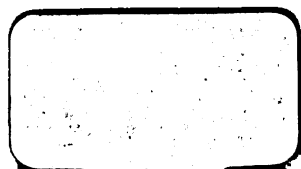
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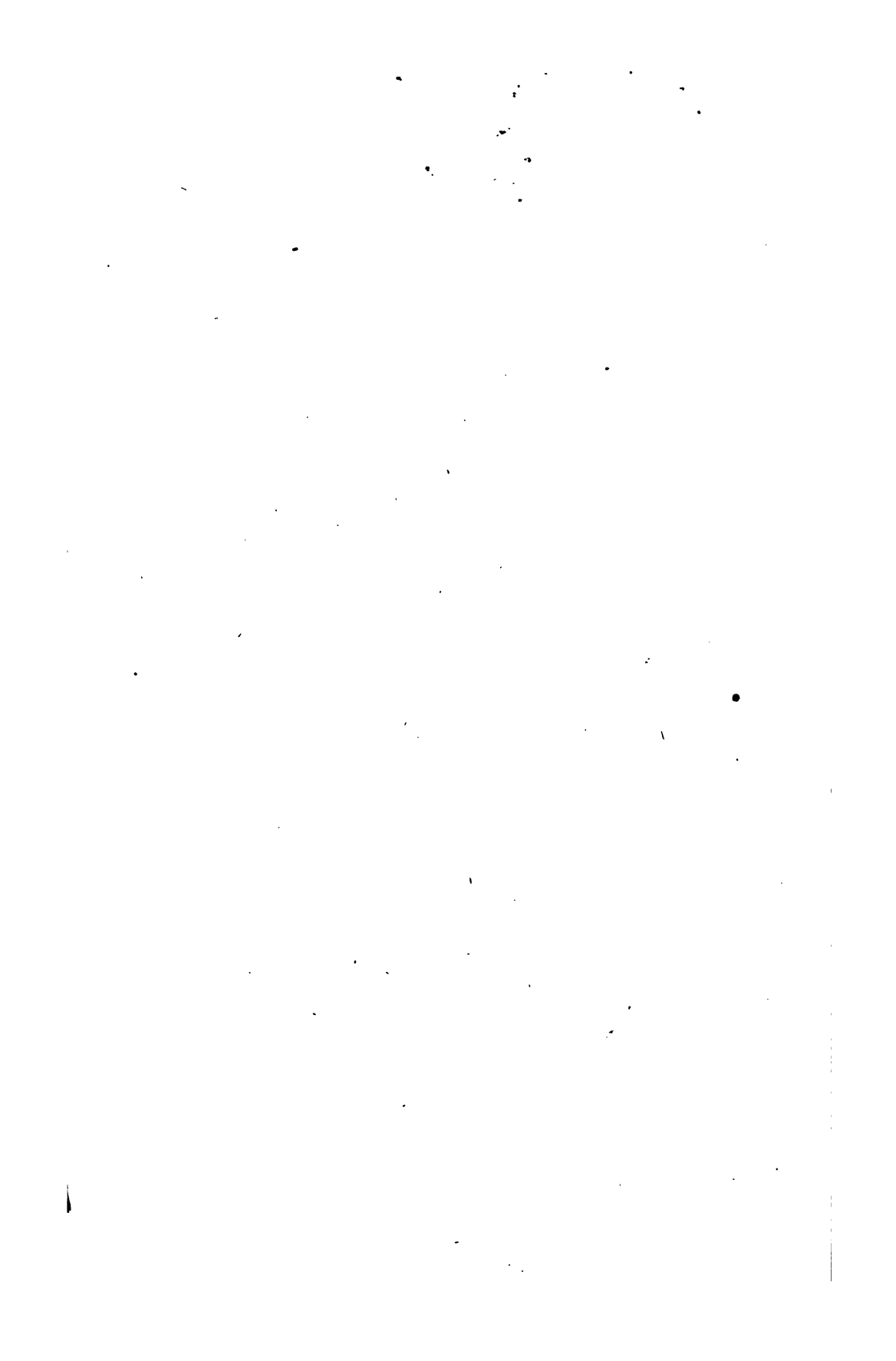
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Physiological
Fallacies.





Physiological Fallacies.

FIRST SERIES.



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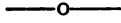
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TO THE ASSOCIATION
FOR THE
ADVANCEMENT OF MEDICINE BY RESEARCH
THESE
PLAIN PROOFS
OF THE
FALLACY AND FUTILITY
OF
VIVISECTION
AS A METHOD OF INVESTIGATION
ARE
PLAINLY INSCRIBED.

CHALLENGE.



ANYWHERE but in England these papers—reprinted for the most part from the *ZOOPHILIST**—would have been out of place.

On the Continent, as Professor Hermann plainly puts it, Science “can afford to despise the justification” of “practical utility.” Here, in practical England, this absolute Divinity of Abstract Science is not yet so fully recognized. The scientist who would here substantiate his claim to over-ride all considerations of humanity and morality must show at least a *prima facie* case of some substantial gain to be thereby achieved.

Hence the Vivisector,—who, in France, Germany, Italy, or Russia, is content to rest his right of torturing at will upon the simple assumption that torture is the royal road to knowledge—must here at least profess that the knowledge to be thus gained is a practically useful knowledge, not only to him but to his fellows whose complicity he seeks.

With ourselves—we say it plainly—this argument has no force. To our, it may be weak, appreciation wrong is wrong and no amount or kind of selfish gain can make it right. If every advance of healing art claimed to have been wrung from the quivering limbs

* To obviate any difficulty in recognizing the force of any argument addressed by a “layman,” it may be as well to mention that of the eleven writers concerned in the production of these papers, nine are fully qualified members of the Medical Profession.

and entrails of helpless animals, since scientific torture first began, were proved, beyond a peradventure, to have been so attained and to have been attainable in no other way, we should still hold that such means were sin; that the gains which sprang from them, be they what they might, would be, and could be, no blessing but a curse.

And, so believing, we find it hard to believe that by such means any real gain is to be got.

We believe in God,—in His Justice, His Mercy, His Love. We do not believe that He so made this world of His as that aught but utmost ill can come to man, His noblest work—to man, made in His Image, bound to His service and training for His Presence—by deliberate breach of His Divinest laws.

In this faith we have grappled boldly with the selfish plea of good to be gained for man by self-seeking cruelty to the helpless creatures God has committed to his power. And it has crumbled in our hands.

We have met the scientist on his own chosen ground of Science. We answer him in his own tongue. Point by point we prove his assertions to be false, his premises unsound, his arguments fallacious, his conclusions fraught, not with good to Man, but with error, and loss, and hurt.

We challenge a reply.

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CHAPTER I.
HUNTER AND THE STAG.

[A LETTER TO PROFESSOR OWEN.]

DEAR SIR,

You will remember that in my letter acknowledging your very courteous reply to my enquiry as to the origin of the story of Hunter and the stag I ventured to draw your attention to a letter of mine about to appear in the *Lancet* in reply to your criticisms of my speech at our Annual Meeting. Curiously enough that letter, which had been twice publicly promised insertion, and was at that moment, as I have reason to believe, actually in type—has since been suppressed. I am therefore reduced to the necessity of directly addressing yourself and need, I feel certain no further apology for so doing.

The criticisms to which I refer were contained in your Address on the unveiling of the Harvey Memorial at Folkestone. That Address consisted, as I need hardly remind you, of an eloquent panegyric upon the hero of the day in his especial character as “the Great Vivisector.” Its text was a phrase of mine to the effect that “Vivisection while it pandered to scientific curiosity added nothing to practical knowledge.” Its argument, an elaborate refutation of that view; firstly by the attribution to vivisectional experiment of Harvey’s own grand discovery of the Circulation of the Blood; secondly, by the story of Hunter and the stag to which I have already referred and for which you claim the credit of having originated the

great surgeon's famous operation for popliteal aneurism. Its conclusion, an earnest appeal for the removal of all restrictions from this method of scientific research.*

In a word you attacked on what you maintained to be scientific grounds a position you stigmatised as one of simple sentiment. On scientific grounds I claim the right to reply. I do not for one moment abandon either the "sentimental" or the moral basis of my proposition. The latter of these at all events is still, I am bold to confess, of higher importance in my estimate

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Home Minister: What is the object, Mr. Hunter, of your proposed experiment on the living deer?

Vivisector: I want to know how their horns grow.

H. M.: And what do you propose to do to gratify that desire?

Viv.: For one thing, I propose to cut down upon the carotid artery, and tie it.

H. M.: And what good do you expect to get by inflicting on an unfortunate animal that degree of pain?

Viv.: I have nothing further in view, sir, than what I have stated.

H. M.: And so you would pander to your curiosity in regard to the growth of its horns by subjecting a poor deer to your detestable operation. I can give no sanction to such inhuman vivisection, of which you are unable to foresee any scientific results in relation to your own professional purposes and practice.

The discomforted physiologist departs: and mankind continue to die of a tormenting malady, sometimes with, sometimes without, the added operation of amputation at the thigh."—*Speech at the unveiling of the Harvey Memorial.*

than any to be found in the whole range of Science, false or true. But it is idle to enter upon argument without some basis of common premiss. The question presents itself to you, and is by you presented to your audience, as one of simple science. As such I am for the nonce content to argue it.

In the present letter I propose to deal only with the question of John Hunter and his discovery. And this for two reasons. First; when a fair discussion of any point is really desired there is nothing like keeping it clear of all foreign topics. I am most earnestly desirous that this question of the gains from Vivisection to "suffering Humanity" should if possible be for once decided not by mere clamorous assertion but by simple scientific argument and proved historic fact. Second; this particular case of Hunter's vivisectional experiment and its assumed result affords, as you have so promptly recognized, precisely the grounds upon which such an argument may most effectively be carried out. You have yourself selected it as a typical instance of the beneficial results of the practice you so urgently advocate. I am quite prepared to accept it as an equally typical example of its utter barrenness. When that question shall have been fairly argued out I shall be quite ready, should you so desire, to deal in similar fashion with your other contention as to Harvey and the Circulation of the Blood. For the present let us confine ourselves exclusively to the story of Hunter and the stag.

How, then, in the first place, does the case stand with regard to this asserted origin of Hunter's invention regarded from the historical point of view as a mere matter of evidence?

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Instead of amputating the man's
 and tie the femoral artery, it
 into the aneurismal tumour
 the blood there to
 ; and if the human
 ervine, a man's leg
 of the popliteal
 (Sign in saying that
 then, is at previous to
 is told it by of treating
 who was at all events that the
 erevertold—from the great sted in
 the invention in question. Surely urred
 basis this, even for a story which D. might and
 himself have had an interest in suppressing?
 this story is precisely one which, had it ever occurred
 to D., would certainly have been told by him with
 special zest. We have his own published account
 of the transaction in question. We have besides the
 still more elaborate account of Sir Everard Home, his
 son-in-law, assistant, *alter ego* and fellow-vivisector.
 And neither account contains, from first to last, one
 solitary word which can, by human ingenuity, be con-
 strued or twisted into the very faintest suggestion of
 any story of the kind.

I ask you, Sir, respectfully but seriously, is this
 third-hand hearsay—flatly discredited by direct and
 indisputable documentary evidence—the kind of basis
 for scientific dogma which you, the Senior Scientist of
 England, are prepared deliberately to accept? Is
 there a conceivable fact, or theory, or assumption,
 however inherently insignificant, at which, resting on
 premises such as these, you would, in dealing with any
 other subject, condescend to cast so much as a glance?

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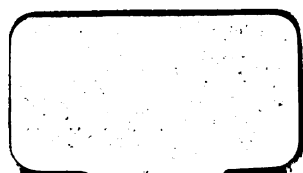
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enough inferred from their general ignorance of the existence of any circulation at all, interfere in any way with the practical dependence upon that collateral circulation of the limbs on which they operated and which knew as much about the philosophy of their own sustenance then as now. Absolute ignorance of the whole circulatory system no doubt gave to Hippocrates as to his predecessors, a boldness in handling the knife which a partial knowledge, covering only the main whilst it missed the collateral circulation, would have lessened if not destroyed. But when the knife was once at work its influence was at an end. If after the obliteration of its main artery any single limb was saved through all the centuries, that limb was saved by the agency of the collateral circulation and by that alone.

And for Anel, at all events, the Circulation of the Blood was part of his professional A.B.C. The one link in the chain still left missing by Harvey, Malpighi's microscope had long since made the common property of every elementary textbook. Anel knew to a line the region nourished by the channel he was about to cut off. He knew just as precisely the result which must inevitably follow were there no other channel at hand by which that nourishment could be supplied. And if, knowing both, he nevertheless proceeded with his operation just as boldly as those who had performed it in blissful ignorance of both those vital facts the inference is surely tolerably clear that he had also a third compensatory knowledge, the knowledge of those collateral channels by which the interrupted circulation might as a question of theory and would as a matter of fact be supplied.

Let us see how far these indisputable and historical

facts justify—or otherwise—the four heads of your proposition. You say :

1. That before Hunter's discovery the only method known of treating popliteal aneurism was by amputation.

ANS. *Aneurisms were treated by simple ligature of the artery scientifically for years—ignorantly for centuries—before Hunter's birth.*

2. That the novelty of Hunter's mode of treatment consisted in tying the artery without removal of the distal portion of the limb.

ANS. *In this respect Hunter's operation, and that at all events of Anel, who died when Hunter was yet a baby in arms, are absolutely identical.*

3. That the basis of this innovation was the discovery by Hunter of the principle of the collateral or supplementary circulation.

ANS. *Anel's operation depended on the collateral circulation precisely as did that of Hunter; who could hardly have made, and could certainly not have communicated its discovery before he was born.*

4. That this discovery of Hunter's was the result of his vivisection of the deer.

ANS. *Omne majus in se continet minus. If the "accident of birth" prevented Hunter from making the discovery at all, à fortiori it could not have been the result of any particular operation of his, even were it one of those early experiments upon the legs and wings of flies in which the scientific proclivities of the embryo vivisector commonly find their first development.*

I think, Sir, I have here shown, with something of clearness (1) that, as I ventured to assert at starting,

you were fundamentally in error with respect to the nature of John Hunter's discovery. (2) That whatever may have been the novelty actually involved in it that novelty was not the substitution of ligature for amputation. (3) That this treatment by ligature was not, and could not possibly have been suggested by the experiment to which you have so confidently referred it and upon its origin in which you based your triumphant refutation of my assertion that vivisection "while it pandered to scientific curiosity added nothing to practical knowledge," and your eloquent appeal for powers of haphazard mutilation of highly sentient animals as unrestricted as those under which the Californian placer-digger drives spade and pick into the soil in hopes of an occasional nugget here and there.

But I have not yet done with John Hunter and his discovery. I undertook to prove not only that, as a matter of fact, that discovery was not based upon the particular vivisectional experiment to which through an entire misconception of its nature you had erroneously attributed it, but that it was essentially of such a kind as to be incapable of aid or illustration from any vivisectional experiment of any description. Let us see then in the second place what this famous discovery of Hunter's really was.

Before his time aneurism had, as we have seen, been treated in two different ways. *First*—let us say*—by amputation. Which, when it succeeded, of course left

* Whether, historically speaking, the "heroic" cure by chopping off the limb and dipping the stump into a kettle of boiling pitch preceded or followed the equally "heroic" treatment by knife and red-hot poker I confess myself unable to determine without more research than, on the whole, the question seems to be worth.

the patient a cripple. And which as a rule did not succeed; the ligature of the stump commonly coming away too soon and the man dying from secondary hæmorrhage. *Second*, by tying the artery close down upon and again immediately below the aneurismal sac, opening and cleaning out the sac itself and trusting to the collateral circulation for the nourishment of the lower portion of the limb. Which operation also commonly failed and from the same cause as the other, the failure that is to say, not of the collateral channels, capillary or otherwise, by which the circulation was to be carried on, but of the ligatures by which it was to have been cut off.

Now in spite of the vivisectional follies into which he was betrayed as naturally as, living a few centuries earlier, he would have followed the Philosopher's Stone, or the Elixir Vitæ, or whatever else might be special scientific wildgoose of the time, John Hunter was a man who thought. And this constant failure in one particular class of cases of an operation the success of which was the uniform daily experience in others, set him thinking. Why did these ligatures, which in a normal condition of the vessel might be placed in precisely the same situation with very tolerable certainty of holding on at least as long as their presence was needed, in contact with an aneurism lose all their holding power, and rapid as the process of obliteration is, come away before it was effected? And so the idea dawned upon him that the failure of the ligature arose precisely from the presence of the aneurism. How? Very simply, when once the solution has been discovered. The aneurism itself is a giving way of the coats of the artery under the pressure of the blood driven through it by the heart. That failure arises from

local weakness ; that local weakness from local disease. In Anel's operation the ligature is placed carefully as closely as possible to the sac and therefore, in all probability, on the diseased patch ; which, having already demonstrated, by the fact of the aneurismal rupture itself, its inability to bear the ordinary pressure of the passing current, is thus called upon to resist the far severer strain of its absolute obstruction by tying.

Go back beyond the diseased patch. Place your ligature upon a sound part of the artery where it is still in possession of its normal powers of resistance and the normal results will follow. In this case the popliteal artery is diseased. Ligature the femoral artery, which is sound, and your ligature may be safely trusted to maintain its hold till the diseased popliteal shall have been finally obliterated and the circulation established through the collateral channels.

This was John Hunter's "innovation," and a brilliant innovation it was. None the less brilliant, I venture to think, in that it was the result not of any mere happy-go-lucky hacking at the throat of an unfortunate stag, but of legitimate argument and logical deduction.

That it was so the conditions of the case itself prove at once "beyond a peradventure." *The whole theory of the improved method is based upon the assumed diseased condition of the affected part.* In the vivisected animal no such diseased condition exists, or can be induced. The only mention of vivisection by Hunter himself in his vindication of his theory is that of an abortive attempt to induce that condition by scraping away the coats of the carotid artery of a dog.

But I will go further even than this. I have shown that with Hunter's brilliant discovery in its original inception

vivisection as a matter of fact had, and as a matter of science could have, nothing whatever to do. Bear with me a moment longer while I show how, in its ultimate development, it was actually directed towards dispensing in great measure with the aid of that collateral circulation to the imaginary accidental discovery of which by Hunter you have erroneously attributed the origin of his invention.

As first designed, the tying of the femoral artery by Hunter's plan was simply, like the tying of the popliteal on Anel's, a first step in the operation, which was only completed by the opening and emptying of the aneurismal sac.* And one of the great objections

* I am quite aware that this statement is open to question. "The ligature of the artery on the cardiac side of the aneurism without opening the sac, was first done," says Erichsen, "by Anel in 1710." And he admits that "this operation, though attended with the risk of wounding or inflaming the sac, which was in close proximity to the seat of the ligature constituted a considerable advance in the treatment of the disease." But he goes on to say that "as Anel however performed his operation as a mere matter of convenience in a particular case and without the recognition of any new principle of treatment being involved in it, it attracted little attention at the time and does not appear to have been repeated by any of the surgeons of his day." Miller—commonly trustworthy enough in the matter of dates—places the operation in 1740, but that, as Anel died in 1730, is probably a misprint. At all events the operation, whatever its inception, seems to have remained altogether in abeyance, on the one reckoning for five and forty years, according to the other and more probable calculation for three-quarters of a century. Then, when in 1785 Hunter makes his grand experiment on the bricklayer at S. George's—an experiment, be it observed, which failed but the failure of which did not in the least disturb John Hunter's robust and well-founded faith in his own powers of deduction—we find him arguing the question *de novo* with an entire ignorance of anything of the kind having been attempted by Anel or by any one else which if not real is certainly

advanced against Hunter's inventions was that it involved the inflicting of two wounds instead of one. This set Hunter thinking again, and gradually he arrived at a conclusion which involved not a mere modification of the accepted treatment, but an absolute revolution; and which, yet further developed by his successors, has led in our day to a system of treatment which dispenses with the knife altogether, and effects its object by simple compression with the finger.

The artery—argued Hunter—has been unable to resist the full strain of the blood pressure; but it has still some power of resistance. Now, suppose that instead of altogether cutting off the current, and relying exclusively on the supplementary circulation for nourishment of the limb beyond, we only apply to the sound portion of the artery just so much pressure as shall reduce the strain to such amount as the diseased portion shall be able to bear. Will not the current, thus slowed, choke up with its fibrous deposit, the mouth of the aneurismal sac? And will not its contents thus left to stagnate, be quietly absorbed by natural process, without local incision of any kind?

To all which questions John Hunter's clear intellect answered, yes; and so he advanced another stage, what has been justly styled one of the most brilliant achievements of conservative surgery. And beyond this he himself did not go. It was left to his successors—I frankly confess that I am not quite certain which of them—to

assumed with most remarkable skill. It is no doubt conceivable that Hunter may have stolen the idea of leaving the isolated tumour to the process of natural absorption from Anel's brachial operation in 1710 but the evidence does not by any means appear to me to bear out the assumption. And even if it were so it was from him that the idea, left fruitless by its originator, received all its practical life and development.

carry out his argument still further and to substitute for the incision and the slack ligature a simple digital pressure on the external surface.

So much then for John Hunter's improvement in the treatment of aneurism ; for its origin in the supposed new discovery of the collateral circulation, and for our indebtedness for that discovery to Hunter's experiments on the antlers of the unfortunate Richmond stag. I leave it to you, Sir, as a man who, whatever his devotion to "Science," is not, I hope prepared, as too many of the common crowd of scientists would seem to be, to sacrifice upon its altar his own candour, honour, and self-respect, to say frankly how far I have proved, or failed to prove, my point ; how far I am justified in claiming, as I do claim, the case you have put forward in refutation of my position as the strongest possible evidence in its support ; in asserting, as I am bold to assert, that that pandering to scientific curiosity with utter barrenness of practical result, which I maintain to be the special characteristic of vivisection, needs and can receive no more unanswerable illustration than this too famous story of John Hunter and the stag.

But one word more and I have done.

We, opponents of Vivisection, are taunted everywhere with ignorance of our subject and incapacity for arguing it. Its advocates are never weary of asserting that the facts and reason of the case are alike wholly on their side. But never yet has a too great confidence in their cause betrayed them even for a moment into the weakness of openly discussing any of the points on which their position is so unimpeachable or of meeting

argument with any riskier answer than assertion and abuse.

I venture, Sir, to hope that in this, as in so many other respects, you will show your superiority to your colleagues. I offer you, with all respect, a scientific argument. Meet it I pray you, as it should be met by a man of science. If it be ill-reasoned, refute it. If it be untrue, disprove it. If it be absurd, demonstrate its absurdity and ridicule it and its author as may seem you good.

But if it have a show of reason or a substratum of fact; if there be in it anything to lead you to suppose that you may possibly have overstrained your own case or overlooked any of its conditions, do not condescend to that "policy of silence" which however befitting to the *Lancet* and the *British Medical Journal* is surely unworthy of one who occupies a position and bears a reputation such as yours.

I am, dear Sir,

Very faithfully yours,

CHARLES ADAMS.

CHAPTER II.

HOPE AND HEART-DISEASE.

WHEN evidence was being offered before the Royal Commission appointed to inquire into the practice of subjecting live animals to experiments for scientific purposes, it will be remembered that long lists of discoveries alleged to have been made by means of vivisection, were submitted to the Commission as positive proof of the great services that vivisection had rendered to mankind. Since then many of the examples adduced have been shown over and over again to be simply misstatements, whose effects were heightened by exaggeration, while other examples still remain unanswered and probably unenquired into. Judging from the examples quoted, it would appear that a general ransacking of medical literature had taken place, and whenever a discovery and a vivisectional experiment were found described in the same work, it was at once assumed that the discovery had followed the vivisection. Even had this been the case, it would not necessarily have followed that *post hoc* was always *propter hoc*. But as a matter of fact, the premises were as inaccurate as the conclusion. When any connection at all could be established between them, it was most commonly the experiment which had followed the discovery, not the discovery the experiment.

It is now our purpose to direct attention especially to the examples which have hitherto been overlooked and unanswered, and to that end we shall commence by inquiring what share vivisection had in revealing to us the causes and character of those diseases of the heart, whose elucidation has been so loudly ascribed to Dr. Hope's vivisectional experiments.

Although this question seems altogether to have dropped out of sight without having been answered, it was considered, at the time of the enquiry, one of primary importance. Professors MacKendrick and Turner both made much of it. The *British Medical Journal* trumpeted forth its virtue under the heading of, "What has Vivisection Done for Humanity?" Even in private drawing-rooms mild anti-vivisectionists were continually being button-holed by obtrusive and ignorant would-be physiologists, and challenged to gainsay the benefits which mankind had received from the experiments in question. We ourselves have had to undergo that ordeal, and being quite unconscious at the time even of the previous existence of the said Dr. Hope, we were forced to plead ignorance of the subject. The positive manner, however, in which the challenge was given had the natural effect of causing us to look through the pages of that gentleman's work "On Diseases of the Heart," and we there found a very different state of matters from what had been represented to us. There were certainly innumerable cases of brutal vivisection to be found there, but the vivisections came not before but after the discoveries. One might have been tempted to think that only a very shallow intellect could have become impressed with the idea that under such circumstances the vivisections had any share in making the discoveries. Yet that

was how they were represented to the Royal Commissioners by the gentlemen we have named.

But let us give the very words themselves. Dr. MacKendrick (Question 3,879) states :—" There is one " interesting experiment, for example, which at once " gave the physician an intelligent comprehension of " the cause of cardiac murmurs sometimes heard over " the heart when you apply the stethoscope over that " region. Certain experiments were made by the late " Dr. Hope, in which he investigated the causes of the " sound of the heart, and he showed that by interfering " with certain of the valves experimentally murmurs " were produced ; indeed, he initiated artificially in a " manner conditions which we know frequently occur in " disease." Again, at 3,916, " (3) The causes of the " cardiac sounds have been determined entirely by " vivisectional experiments."

Again, Professor Turner remarks (3,027) :—" The " experiments of Drs. Hope and Williams are of " importance in determining the cause of the sounds of " the heart and in enabling the practical physician to " diagnose certain of the diseases of that important " organ." There is no ambiguity in the foregoing sentences ; the wording as well as the intention is to show that the recognition of the diseases of the heart was due to experiments, and that vivisection was Dr. Hope's *modus operandi* in investigation.

But the facts, as stated by Dr. Hope himself, are exactly the opposite, and are published in the third edition of his work, page 28. He says there :—" Con- " scious of the gap that was presented in the treatment " of diseases of the heart, I have devoted more attention " to this than to any other department of the subject, " availing myself, in particular, of the wide and favour-

“able sphere for observation afforded by a long residence as house physician and surgeon in the Royal Infirmary of Edinburgh, where, living literally, I may say, as well as figuratively, at the bedside of the patient, I had an opportunity of closely watching every habitude and phase of the disease, every operation and effect of remedies.” The bedside of the patient and not the side of the torture-trough was where Dr. Hope made his investigation. But if any doubt remains unexplained by the foregoing, he makes it clear further on at page 76, where we find the following:—“Mitral Valves.—(1.) Systolic murmur, that is from regurgitation. It was the existence of this murmur in Christian Anderson” (a patient in the infirmary), “who had no disease of the semilunar valves, that led me to the detection of regurgitation in general, in June, 1825.” Here, then, from Dr. Hope’s own account we learn the date, the cause, and character of the lesion,—with the name of the patient (not an unfortunate dog)—which opened out to Dr. Hope his great discoveries.

What happens subsequently is clear enough to any medical man. With the knowledge acquired from long clinical studies in the hospital Hope comes up to London. But let him announce his discoveries as he may, no one will accept them unless he is prepared to demonstrate in some way their causation on living animals. This is always a stumbling block for any young observer, who must use the shibboleth *proved by experiments on animals* to make his theories acceptable to the heads of the profession. His discoveries were specially connected with diseased conditions and abnormal sounds which are not generally found in the animals usually experimented upon, and so experi-

ments are devised that will imitate as nearly as possible the diseased conditions. The consequence is that doubt and differences show themselves amongst the doctors who witness the vivisectional experiments. There is all the difference imaginable between registering carefully the character of the sounds of the diseased heart in a patient where one may listen and come again a hundred times over, afterwards comparing them *post mortem* with the lesions which have led to such abnormal sounds, and the chance lesions, coupled with convulsive sounds and actions of the heart, in the poor tortured dog or ass.

Moreover, Dr. Hope himself shows that he puts little faith in the result of such experiments. The notorious vivisector Majendie had taken exception to some of the views held by Hope, and "had adopted a kind of alternate theory of the heart's movements. If, said he, the heart of a living animal is denuded, we easily see the auricles and ventricles contract and dilate alternately."

To this Hope replies—in somewhat singular phrase for a man whose discoveries are made by means of vivisectional experiment:—"It is easy to see how M. Majendie has been misled, namely, by operating upon *living* animals" (the italics are Hope's), "for I have always found that when animals retained or regained the slightest degree of sensibility, the action of the heart was so violent, convulsive, and rapid as to present the appearance of alternate action described by Majendie."

It is scarcely credible that the man who penned the foregoing lines should be put forward as a believer in the *Vivisection which has done so much for Humanity*. His

arguments are precisely those used in general by anti-vivisectors like ourselves, and his provivisecting admirers must surely find some difficulty in reconciling his statements with their own pretentious assumptions on his account.

CHAPTER III.

ASELLI AND THE LACTEALS.

THE discovery—so-called—of the lacteals by Aselli in 1622 is commonly put forward as one of the most unanswerable proofs of the value of vivisection as a method of research.

The “lacteals” we may observe for the benefit of our non-scientific readers are that portion of the lymphatics which convey the chyle, or nutrient product of the digested food from the intestine to the blood. Their peculiarity is that it is only when actually employed in the transmission of this milky fluid that their otherwise transparent structure becomes visible to the naked eye. And this is only during the actual process of digestion ; and that, of course, only in animals the nature of whose food gives to its digested extract this peculiar milky appearance.

It is the stock case always held out to students of physiology when the teacher launches out against the ignorant and fanatical anti-vivisectionists ; and of course it was duly quoted before the Royal Commission. Professor Turner introduced it there (Qu. 3,025), as one of the “three subjects for illustration, because they lie at the very foundation of all our present physiological knowledge. Without them,” said the Professor, “physiology and consequently practical medicine would be a perfect chaos.” After such an introduction no one can suggest that we

choose either an obscure or worthless example for enquiry, and we shall use Professor Turner's own description as our text. He says (page 158): "The next subject for illustration that I wish to bring forward is the discovery of the important system of vessels known as the lacteal vessels, or the lymphatic vessels, vessels which are concerned in the absorption of the food and in the process of nutrition. In short, the observations and discoveries on these vessels lie at the very root of our knowledge of digestion and the assimilation of food. The first observation on the discovery of the lacteal vessels was made by the Italian anatomist, Asellius, in the year 1622. I may explain that these vessels are of a very remarkable character; they are as a rule extremely minute; their coats are so transparent that they cannot be seen excepting when they are filled; they must be filled either with the chyle which they convey from the bowels, or by some artificial means. The best means of seeing them, is when they are filled with their natural fluid, that is the chyle, and their chyle contents are only to be seen when digestion is going on. After the food has passed away from the bowels the chyle is no longer in these vessels; they must be seen therefore when digestion is going on.

"Now Asellius discovered these vessels by opening the body of a living dog at the time when it was digesting food. But although Asellius discovered them, he did not entirely comprehend what their direction and course was. He supposed they ended in the liver. It was reserved for the French anatomist, Pecquet, in 1649, to determine that these vessels terminated in the great veins at the root of the neck," and so on.

Now what are the facts? In the first place Aselli did not discover the lacteals at all. He simply re-discovered them. These vessels were known to, and described by anatomists of the School of Alexandria, eighteen centuries previously. Erasistratus described them, as he had seen them in the mesentery of sucking kids, as "canals full of milk," and as far as we can learn that discovery of Erasistratus was purely an anatomical discovery, made, not by the aid of vivisection, but by the careful use of the scalpel upon the dead body. Herophilus also described the same vessels, but he considered them to be veins just as Erasistratus supposed them to be arteries, and Asellius at first supposed them to be nerves. During the centuries which passed after the two former anatomists had passed away the knowledge of their discoveries had been forgotten, although in 1563, Eustachius discovered in the horse the main lacteal called the *thoracic duct*. And this discovery again must have been a purely anatomical discovery, as in that animal there is no milky chyle, and vivisection, the only possible advantage of which would be in the exposure of these vessels while actually engaged in the process of digestion, and so marked out to observation by the thick white fluid with which they are then filled, would of course have been of no use whatever.

After these anatomists came Aselli, who in the manner described, re-discovered the lacteals, when in pursuit of another investigation, he opened a living dog. And here, if anywhere—in this simple re-discovery of what had already been again and again discovered not by vivisectional "research" but by legitimate anatomical investigations—we must look for that justification of vivisection for which we are so triumphantly

referred to this all-important "discovery." And once again,—What are the facts?

The dog in whose intestines the discoverer found the lacteals gorged with their distinctive milk-like contents had been opened alive in pursuit of a vivisectional experiment. But the search for the lacteals had no more to do with the opening of him than the search for the North-West Passage or for the Philosopher's Stone. Aselli's mind was occupied, not with the working of the digestive organs but with the movements of the diaphragm. But the dog happened to have just finished a meal, and a meal with fat of some kind in it, and the observer's attention was accidentally arrested by a phenomenon *which would present itself in precisely similar fashion in the intestines of any dog, or swine, or man opened during the process of digestion alive or dead as in point of fact it was observed only two years after Aselli's death by Gassendi in the dead body of a criminal opened an hour after his execution.* And the result is trumpeted as a triumph of vivisection—just as the burglar who has happily effected an entrance by the window of a room the occupant of which happens to have set his curtains on fire will, no doubt, for ever after stigmatise the eighth commandment as an obviously immoral restraint upon the saving of life.

And now, when accident has placed the vivisector upon the track of an important discovery, how does he follow it up? A very few hours of patient work with the scalpel—a very simple exercise of ingenuity in injecting the newly discovered vessels with some distinctively coloured fluid which should mark them plainly throughout their course—and that course would have been traced by the physiologist Aselli, as it was subsequently traced by the anatomist Pecquet

first to the thoracic duct and thence to the veins at the root of the neck. But then the physiologist Aselli would not have been acting as a physiologist. So instead of investigating he set to work to experiment and to theorise. Animal after animal was sacrificed and the only result attained an ingenious but perfectly erroneous theory which traces the newly discovered vessels to the liver, with which they had no more to do than the Rhone with the North Sea. Other eminent vivisectionists—the great Harvey among the rest—joined in the quest and succeeded in demonstrating, not the true bearing of Aselli's discovery but its chimerical nature altogether ! For had they not cut open scores and hundreds of animals which, feeding upon herbs did not transmute their food into milky chyle and in which consequently the lacteals could not be seen and were therefore of course non-existent ? Vivisection had accidentally blundered upon an important discovery. Vivisection promptly recovered from its accident and triumphantly proved the discovery a blunder. And so the "discoverer" Aselli died—in his ignorance of the true bearing of his discovery. And twenty years later the anatomist Pecquet blew the theories of the vivisectionists to the winds and freed science for ever from one more set of the "errors" which experimental Physiology had done its best to "perpetuate."

CHAPTER IV.

RUTHERFORD AND THE LIVER.

PROFESSOR RUTHERFORD is, as everyone knows, one of the leading lights of English, or rather Scotch, physiology. Needless to say that he is an enthusiastic advocate of the perfect freedom of that invaluable institution the vivisectioning-trough. According to his evidence before the Royal Commission—"The great opprobrium of medicine at this moment is the indefiniteness of our knowledge regarding the action of medicines" and it is evident, he thinks, to all that "the proper way to diminish this ignorance is for the physiologist to perform experiments upon the lower animals and then to submit his results to medical practitioners, so that they may obtain therefrom suggestions regarding the employment of these medicines on man."

Dr. Moxon is, like Professor Rutherford, an enthusiastic believer in the virtues of vivisection, and he is one of the medical practitioners who, as both he and Professor Rutherford proclaim, must needs, from the abstract point of view, benefit so largely by the Professor's "researches." And this is his view, as delivered in his Hunterian Oration, of their practical value when presented in the concrete form of the precious investigation by the Professor and his committee into the action of the liver. "The question was," says Dr. Moxon, "whether mercury is useful in moving the bile.

“ The practical question before that committee was
 “ whether mercury would unload engorged human
 “ livers. To settle this they took *healthy* dogs, and,
 “ regarding lightly the vastly important connections of
 “ the duct or pipe which carries bile out of the liver, this
 “ duct—with its neighbouring vessels and nerves, and
 “ its wonderful structure—they cut it boldly through,
 “ and brought it to the surface, and fastened it to the
 “ skin outside, and then they gave the dog mercury ;
 “ and they say it did not unload him of bile. Now, I
 “ can fancy that Mr. Mill would grin rather solemnly
 “ at that experiment if he were fond of dogs. For, first,
 “ as to unloading the dog’s liver, it was not shown
 “ to be loaded at all like the engorged human livers of
 “ the *practical* question are loaded, which loaded livers
 “ mercury is thought to relieve. And Mr. Mill might
 “ ask, ‘ What did these people think we meant when
 “ we said mercury unloaded the liver ? Did they think
 “ we meant that it squeezed it, as in a cheese-press ?
 “ Or what force did they think the mercury would
 “ use that it could act in spite of all this rudeness ?
 “ Does experience lead us to think biliousness is
 “ determined by very rough measures. Some people
 “ grow bilious on sherry, and not on port, and if
 “ the bile is determined by such trifles, could it be
 “ indifferent to the dragging of the bile-duct from its
 “ supremely important connections, and cutting and
 “ tying it ? Surely it is possible that it is along this
 “ pipe that the mercury acts, so that its channel was
 “ cut off.’ And Mr. Mill might say what I would not,
 “ and ask whether a simpler experiment would not
 “ have answered better. He might ask whether, if
 “ this committee, instead of hardening their hearts and
 “ sharpening their knives, had met in a protracted

“ dinner-party to quicken their brains and thoroughly
“ engorge their livers, and had then each taken a blue
“ pill, and in due time held another committee, the
“ experiment would not have been of less boastful
“ quality, more truly reasonable, and might not have
“ ended in establishing mercury more firmly than ever
“ in its almost proprietary command over the British
“ liver.”

The real vivisectors—the men who understand their business and to whom our English “physiologists” look up as their masters in the gruesome science—make of course no pretences of the kind. Like Bernard they acknowledge frankly, and with some degree of amusement at the question, that so far as practical benefit to medicine is concerned their hands are empty ; or like Majendie protest, with the emphasis of a man defending his character for common sense, against the absurdity of supposing that any physiologist would dream of calling to his own bedside a physician whose knowledge had been gained from the vivisection trough. But then, on the Continent, they can do what they like and there is no need of an appeal for impunity to the ignorant selfishness of their countrymen. The English physiologist gives his brother Englishman credit for objecting to cruelty—unless where he thinks there may be something to be gained by it. And sets up the cry of “Suffering Humanity” accordingly. Only sometimes the grim joke goes a little too far. With results as above.

CHAPTER V.
OWEN, HUNTER AND HARVEY.
[A LETTER TO THE ASSOCIATION FOR THE ADVANCE-
MENT OF MEDICINE BY RESEARCH.]

Before proceeding to the consideration of the second case put forward by Professor Owen, in confutation of my remark that " Vivisection, while it panders to scientific curiosity, adds nothing to practical knowledge " a few lines are needful as to the issue already raised. Fortunately very few will suffice.

And first, a word, not exactly of apology but of explanation. Knowing what I now know I cannot but see that the personal appeal with which I concluded my former letter must have worn in Mr. Owen's, and possibly in other eyes, an air of irony.* Nothing could be

* The following is the passage in question :—" I leave it to you, Sir, as a man who, whatever his devotion to ' science,' is not, I hope prepared, as too many of the common crowd of scientists would seem to be, to sacrifice upon its altar his own candour, honour, and self-respect, to say frankly how far I have proved, or failed to prove, my point. . . .

" We, opponents of Vivisection, are taunted everywhere with ignorance of our subject and incapacity for arguing it. Its advocates are never weary of asserting that the facts and reason of the case are alike wholly on their side. But never yet has a too great confidence in their cause betrayed them even for a moment into the weakness of openly discussing any of the points on which their position is so unimpeachable or of meeting argument with any riskier answer than assertion and abuse. I venture, Sir, to hope that in this, as in so many other respects, you will show your superiority to your colleagues. I offer you, with all respect, a scientific argument. Meet it I pray you, as it should be met by a man of science. . . . Do not condescend to that ' policy of silence ' which, however befitting to the *Lancet* and the *British Medical Journal* is surely unworthy one who occupies a position and bears a reputation such as yours."

"dinner-party to quicker
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 any such proposition as it attributes to me
 de
 sole argument of the pamphlet in question is based
 first to last upon its negation.

2. Mr. Owen then falls back upon a simple re-assertion
 of his original position, and here perhaps the best reply
 will be, to quote as it stands an utterance which I
 frankly admit to be beyond my powers of reply, for the
 plain reason that it is far beyond my powers of
 comprehension.

How came Hunter, he asks, to make his great
 discovery? And he answers thus:—

"The able and devoted assistants in his experiments and prepara-
 tions well knew, and imparted that knowledge, when, in the course
 of my work descriptive of the Hunterian Physiological Collection, I
 found, besides the dry injected specimens, including that of the
 cured popliteal aneurism which Hunter obtained, long after the
 subsidence of the arterial tumour, on the death of his patient from
 another disease, that no fewer than twenty-four preparations
 (Nos. 163 to 187 inclusive) were defined in the scrap of MS.
 catalogue which Hunter had left respecting them, as exemplifying

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doubt?

that new canon of
"us" was he led to conclude—
—that Hunter's cataloguing his
illustrative of the growth of a stag's antler
to have regarded them as the basis of a cure for
human malady? By what peculiar process of arith-
did he arrive at the conclusion that a series of twen-
four could possibly extend "from 163 to 187 inclusive?"
And above all, what—or who—was the medium of
communication with those "able and devoted assistants"
of whom, he assures us, Mr. Clift was "the sole
survivor?"*

3. Finally Mr. Owen resorts to that *ultima ratio* of our
first childhood, that primæval polemic familiarly known
as "calling names."

Into that line of controversy I must beg to be
excused from following him. Every man must be of
course the judge of what most accords with his own
dignity. It seems to me that mine will be best
consulted by leaving the venerable gentleman in this
one respect master of the field.

Passing on to the matter more immediately in hand,
Mr. Owen's second illustration of the fecundity of the
vivisectional method of research, we seem to be met at

* This last is a point to which I would more particularly direct
the attention of the Association for the Advancement of Medicine by
Research.

the outset with a misapprehension as to the very nature of the discovery itself even more astounding in Harvey's case than the similar error in that of Hunter.

I say "seem," for, as I have already admitted, there are times when our author's utterances soar beyond me. Here at all events are his own words as communicated to the *British Medical Journal* of the 13th August last.

"This announcement of the 'lesser circulation unknown to physiology for more than a century after its record, became a lasting possession in human knowledge by Harvey's independent researches; in connection with that of the true nature and way of work of the whole cardio-vascular system—heart, veins, arteries of every part of the human frame—not merely the 'circulation of the blood,' but of its two-fold circulation, and this not by a new or different interpretation of structure, but by visible demonstrations of functions.'"

Now surely if this mean anything—as I think one has fairly a right to assume it must, more or less—it means that the greater or systemic circulation being already a recognised fact Dr. Harvey supplemented it by his own original discovery of the lesser circulation through the lungs. Now this, though not precisely the reverse of fact is yet curiously near it.

In Harvey's time the pulmonary circulation was still no doubt to a great extent a subject of dispute and the first seven chapters of his great essay *De Motu Cordis*, &c., is taken up with the demonstration of this theory. And having completed this demonstration, he tells us that thus far he has "spoken of the passage of the blood from the veins into the arteries and of the manner in which it is transmitted and distributed by the action of the heart ; points to which some *moved by the authority of Galen or Columbus or the reasoning of others* will give their adhesion."*

Now these are not exactly the terms in which a man

* Harvey's Works, page 45.

speaks of his own altogether original invention. Nor again, when he tells us that :—" From Galen, that great man, that father of physicians, it clearly appears that the blood passes through the lungs from the pulmonary artery into the minute branches of the pulmonary veins, urged to this both by the pulses of the heart and by the motions of the lungs and thorax."*

But the lesser circulation thus disposed of he passes to his own discovery—the great discovery which so far from having been already demonstrated by Galen or Columbus is of "so novel and unheard of a character" that he not only "fears injury to himself from the envy of a few, but trembles lest he has mankind at large for a detractor"—the grand discovery of the "greater or systemic circulation," the passage of the blood not "from the veins to the arteries" but from the arteries to the veins.

It is not upon the authority of Galen or of Columbus that we are invited to accept our author's conclusions now. The discoverer is speaking for himself and in the simplest and plainest phrase we learn how "surveying his mass of evidence" and "long revolving in his mind" he at last "began to think whether there might not be a motion as it were in a circle." And this, he placidly adds, "I afterwards found to be true."

This then is the discovery with which we have to deal. Was it one which (1) as a matter of fact was made, and (2) as a matter of science could only have been made by means of vivisection? To both these questions I think I shall be able to supply, as in Hunter's case, a direct negative. As in Hunter's case also, I think I shall be able to prove even more than

* Harvey's Works, page 44.

this—that this grand discovery not only need not have been made and was not made by these means but that it was physically impossible that it should be so made.

And here let me call attention to a fact which would seem singularly but constantly to escape the notice of all the numerous champions of Vivisection. They speak of Harvey's supposed vivisectional discovery as we in my youth of George Stephenson and the wonderful run from London to Birmingham in less than five hours. They seem to be under the impression that vivisection was a happy thought, hidden from the darkened mind of man till suddenly evolved by the brilliant boy of Folkestone, and then instantly revealing the great secret after which the mighty intellects of the past had, without it, been toiling in vain. They forget that it had been, certainly for 2,000 years, probably—according as we estimate our chronology—for twice or thrice or twenty times that period, the chief, as it was no doubt originally the only method of scientific research. If you want to know what goes on inside of anything, be it drum or be it thorax, cut it open and see; is the first instinctive "scientific" formula of the savage or the child. So far from having even developed since those earlier days, its practice had been narrowed and hampered to an extent to which the very small further restrictions since imposed are an addition scarcely appreciable. Harvey no doubt was free to cut up living dogs and cats without the necessity of first applying to an official inspector however friendly and accommodating. But the great truth so boldly proclaimed by Caiaphas and Sir W. Gull was as unrecognized in Harvey's time as now. That terrible foe of "Science," Christian civilization, had been doing its work even

then ; slowly enough but yet with some exactness, and the operations of the English vivisector were already restricted to "the lower animals." The Italy of Fallopius occupied much the same position in relation to England which the Italy of Mantegazza occupies now. Professor Lister, had he but had the happiness to live in those days, would not have cut short his late scientific trip at Lyons, and contented himself with "researches" in a few horses or mules. He would have gone on to Florence or Pisa, and got a Government grant of half-a-dozen human "criminals"—demagogues, heretics, indiscreet nuns, or what not—who would have been really worth the journey. A dozen or so of centuries further back still—with Jews a drug in the market, and Christians the natural sustenance of the menagerie, and the only vexed question as to the feeding of lampreys whether they fattened best on white slaves or black—the Professor Ferrier of the period would never have wasted his money on "highly intelligent monkeys," when at half the price he might try his red-hot wires upon the brains of as many learned Rabbis ; or Mr. Ernest Abraham Hart have devoted the pages of his *Journal* to records of crucified frogs when every sentence might be illustrated with cuts from the living organs of some "crysombed child." But by Harvey's time the English human subject was under protection—his hands were so far tied while his predecessors had been free.

Vivisection then had had it all its own way for, let us be moderate, and say 2,000 years. In whatever respects Harvey's practice of, or dependence on it differed from those of Galen or Hippocrates the difference was solely in that, in his case, the one was less free and the other less complete. I maintain—and my contention

is based as in Hunter's case, in the first place on the nature of the discovery itself, in the second upon the account of that discovery by the inventor—that it was precisely in these restrictions that his superiority lay. That he succeeded where others had failed, not because he was a greater vivisector than they, but because his clearer and more powerful intellect was less liable to be misled by the fallacies of that mistaken system, and less absolutely under its control. That in point of fact his discovery was made not by the aid of Vivisection but in spite of it.

Once more then—what are the facts?

The circulation of the blood is twofold. The lesser or pulmonary circulation, from the right ventricle of the heart, through the pulmonary artery to the lungs and back through the pulmonary vein and left auricle of the heart to the left ventricle; the greater or systemic circulation, from the left ventricle through the aorta and its branches into the capillaries and thence through the venous system, the vena cava and the right auricle back again to the right ventricle. The whole process has its analogy in the grander operations of the purification and fertilization of the soil. The ocean is the heart; the clouds the lungs; the showers the arterial; the rills, and brooks, and streams and rivers the venous system. Substitute for the showers a second riverine network, with its cloud-purified waters driven backward from river to brook, and brook to rill by the action of the sea, converted into a huge double force-pump, and performing mechanically the work now entrusted to successive evaporation and condensation, and the analogy becomes precise. Properly speaking the sequence here given is inverted; the pure and nutrient

blood starting from the left ventricle, depositing in its systemic course its fertilising ingredients and accumulating in their stead a mass of waste products ; and in its pulmonary journey travelling to the lungs, there to be purified of its contaminations and re-impregnated with its vitalizing quality. I have taken them here not in their physiological but their historical order. In Harvey's time the lesser or pulmonary circulation was as we have seen a partially established fact. His grand work was the demonstration—in all but one final link at which to the last he could only guess—of the greater, or systemic.

And this work, even more plainly if possible than the work of Hunter in dealing with the failure of previous operations for aneurism, was essentially one of thought. It proceeded of course upon a basis of established but isolated facts, each and all of which individually, might possibly have been established, some of them even, let us grant, conceivably suggested, by Vivisection ; but each and all of which, with one solitary exception, had already been established, most of them hundreds, some of them thousands of years before his time, by proofs wholly independent of any such corroboration. The impervious septum ; the valves which at entrance and exit of auricle or ventricle intercept, here the ingress, there the egress or the regress of the blood-current ; the jets which spirt from every severed artery in rhythmic cadence with each impulse of the heart ; the invariable dilatation induced by compression, in every vein upon the distal, in every artery upon the cardiac side ; the identity of characteristic between the arterial blood and that in the left ventricle and between the venous blood and that in the right

ventricle of the heart ; the muscular structure of the heart itself, were each and all the fully established conditions of the problem with which he had to deal ; had each and all been proved—were each and all in daily course of demonstration—in the routine experience of the operating theatre and the dissecting-room without the need of any vivisectional experiment of any kind. The one comparatively recent addition to this store of facts—a pregnant addition in its results on Harvey's mind though not really contributing to the argument very much more than had already been contributed by the valves of the right auricle—was the discovery by Harvey's own master, Fabricius of Aquapendente,* of the valves distributed throughout the veins. A discovery like the rest, purely of the dissecting-room.

And if the isolated conditions of the problem were thus effectively set forth without the aid of the vivisector's knife, its synthetic solution was a matter altogether beyond its reach. King Blood might, "go a progress through the veins and arteries of a beggar" and tokens of the regal presence might be manifested here and there. But to have the whole Royal route laid bare from end to end would be more than the vitality of the sturdiest mendicant could endure. Long before any given drop or teaspoonful of blood could have been traced—if indeed it conceivably ever could be traced—at the scalpel's point from ventricle to aorta, from aorta to iliac, from iliac to femoral, from femoral to popliteal, from popliteal to tibial and so back

* The claim of Fabricius to the merit of this discovery is (of course) disputed. Harvey himself speaks of him as the first who "gave a representation" of the valves in question.

in ever purpling course to vena cava and auricle once more, the most enduring colley that ever resigned his honest carcase to the vivisectioning-trough would have tacitly abandoned his passive share in the investigation and left the investigator *planté là*, without any blood-current to trace.

And in perfect accordance with these very obvious facts is Harvey's own account of his own discovery. I do not pretend to say that like Hunter's history of the popliteal operation it proceeds altogether on the lines of philosophic argument without any trace of empiric experiment. Quite otherwise. The cruel fallacy which was the recreation of Hunter's scientific leisure, but the trammels of which his stronger temperament flung contemptuously aside the moment they began to hamper his practical work, was to Harvey the one only recognized road to knowledge. For him a physiological discovery could no more be accepted without some at least plausible vivisectional basis, than, when so provided, it could be submitted to his scientific colleagues in any less scientific language than that by aid of which their successors still invest with the wonder-working charm of mystery their simplest prescriptions of *Pil. Pan.* and *Aq. Pumpaginis*. He lays it down as a canon of scientific enquiry that "the senses" alone are the ultimate appeal and that though, when sensible demonstration is impossible the enquirer must needs be content with reason—"as he who enquires into the cause of an eclipse must be placed beyond the moon if he would ascertain it by sense"—yet that this gives very inferior results and "the example of astronomy is by no means to be followed." And so the conclusions of his clear insight and logical thought are resolutely held at arm's length till they can produce

at least some sort of witness from the senses.* To a Catholic sympathy, broad enough to embrace what it abhors and realise the human element even in inhumanity, there is something absolutely touching in the desperate perseverance with which he seeks to drag from his unresponsive feticch the revelation which his own clear brain had already afforded him. Like Baal's priests on Carmel, he cries aloud, and cuts "with knives and lancets"—not himself indeed but whole hecatombs of hapless animals of every grade. But there is no voice nor any that answers. Here and there perhaps a solitary experiment—like that for instance upon the heart of the snake—adds some slight corroboration to a fact thoroughly ascertained and proved before. But even these are either so deficient in the analogy of their original conditions or so gravely compromised by the disturbing influences of the experiments themselves, as to be practically almost as irrelevant as they are unquestionably superfluous. Let his great Essay be submitted to any impartial person, accustomed to deal with evi-

* The relation of Vivisection to true Physiological Science is very much that of Astrology to Astronomy. The prominence assumed in Harvey's great Essay by his vivisectional efforts, and the amount of practical bearing exercised by them upon his argument, may be not inaptly illustrated by the journal of some scientific naval captain, who having from careful study of tides, currents, winds, &c., as recorded in the logs of previous voyagers, succeeded in mapping out a practicable North-West Passage, should feel bound to justify each day's work on orthodox astrological principles. The proportionate space occupied by the half-dozen logarithms from which at every noon his position on the chart is pricked, and by the sheets of hieroglyphics by which he would nightly demonstrate the oppositions of Saturn, favourable conjunctions of Jupiter and Venus and disturbing influences in the Houses of Mars, Bacchus, Apollo and Virorum by which it had been predicted, would be very much that of the argumentative to the experimental portion of the Essay. The relative value would be identical.

dence; to any judge or committee of judges, on the Bench; and let them decide whether in this matter Harvey learned any single thing from all the dogs he ever tortured—except perhaps the Latin in which he recounts the torments to which he subjected them.*

I will go even further than this.

It is the fashion to speak of Harvey's great treatise as a model of plainness and lucidity. I venture to think that those who thus speak, speak from hearsay and have never read it for themselves. Full of clear thought and cogent argument it is, and couched every here and there in language as clear and as cogent as either. But only here and there. The general effect is involved and confused. So confused and so involved that strange as it may seem neither our Professor nor Harvey's own translator have been able to carry away a clear idea of his meaning.

In the inevitable enthusiasm of the biographer, for example, Dr. Willis not only maintains with, I think perfect justice, Harvey's claim to the honour of independent discovery without any suggestion from the previous but unpublished work of Servetus but valiantly denies that Servetus made any discovery at all.† But whilst thus bent

* And which seems sometimes to have puzzled even his translator. Take for example our author's statement ". . . . è cavitate pectoris . . . pus . . . per arterias, cum urinâ vel cum fecibus alvi, posse expelli." Which the Sydenham Society "translates" into the startling assurance that *all three of the superfluities in question can be conveniently discharged through the arteries!* —Page 18.

† As a matter of fact Servetus not only does describe the pulmonary circulation in the plainest terms, but on one important point is right where Harvey is wrong. The effete blood says the latter "returns to its sovereign the heart . . . there to recover its state

on unduly exalting his hero at the expense of Servetus, he curiously fails of doing him bare justice in the case of Malpighi. Harvey, he tells us "had no notion of the one order of sanguiferous vessels ending by uninterrupted continuity or by an intermediate vascular network, in the other order."* Yet, speaking of the passage of the blood from artery to vein at the extremity of a limb, Harvey not only suggests in so many words "an anastomosis of the two orders of vessels" as an alternative to the assumption of "pores in the flesh . . . permeable to the blood,"† but puts the former suggestion first, not inconceivably, I think, as being in his eyes the more probable.

Mr. Owen's misreading of his author is more striking still. "Harvey" he tells us "showed his hearers and watchers that the organ became erect, and gave the beat which we feel upon the chest; then that it contracted, became notably shorter and narrower."

Now the "movement of the heart" was precisely the one thing which Harvey's vivisections really did, not indeed suggest but demonstrate. And the demonstration was precisely the reverse of that which the apostle of Vivisection here attributes to it. The notion that, as Mr. Owen tells us, it is the diastole or expansion of the heart which gives the beat and that the beat thus given is followed by its systole or contraction is precisely the popular error which Harvey sets himself to refute. "It is generally believed," he reminds us, "that when the heart strikes the breast . . . the heart is dilated . . . but the contrary of this is the fact, and

of excellence or perfection. Here it resumes its due fluidity . . . and is impregnated with spirits," &c. "*In the lungs*" says Servetus on the other hand "does the mixture take place."

* Page xli.

† Page 58.

the heart *when it contracts and the shock is given*, is emptied."*

Of course I do not for a moment pretend to say that, as concerns the actual physiological fact, Mr. Owen is not right and Harvey wrong. When two such doctors disagree so flatly, it is not for a mere outsider like myself to take upon him the decision. Nor is the decision one in which, from this particular point of view, I take any very special interest. My present concern is not with the result of the inquiry but with its method and here the bearing of this curious little discrepancy is the same in either event. Whether it were the immortal Harvey who bungled his experiment or the venerable Owen who blundered in his report, the pit into which either giant fell was equally dug for him by Vivisection. To Vivisection accordingly I am enabled with much gratification to attribute at length this one practical result; a result significant at all events, even if not strictly satisfactory.

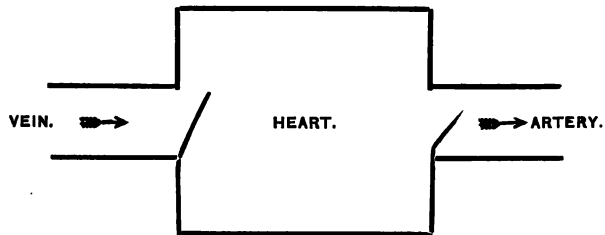
Significant more especially, I venture to think, of its general influence on the whole course of the argument. We will not waste time on admittedly unsatisfactory experiments. Let us take, as crucial tests of the futility or otherwise of this mode of investigation the two experiments expressly recorded by Harvey as having almost alone furnished a thoroughly successful result.

So bewildering, he tells us at starting, were the movements of the heart as studied under the somewhat abnormal conditions of a dog's thorax violently broken open, with flayed chest and ribs notched and snapped off one by one for the purpose, that he "began to think with Frascatorius that the actions of

the heart were only to be understood by God." Only in the last few faint beats could he trace any distinct sequence at all. Fortunately the sequence of movements so traced agreed with his preconceived and perfectly accurate views and Harvey was happy.

Suppose that instead of agreeing with his views these same observations had been diametrically opposed to them? Need his happiness have been a whit the less? Not at all. The explanation would have been simple. "This special sequence of movement," we should then have been told, "is observed only immediately before death, which follows it as a matter of course. It is thus abundantly clear that, even if this particular sequence of movement be not the direct cause it is the invariable precursor of death and must therefore presumably differ essentially from those which are the support of life. Q. E. D."—as before.

The heart of a snake, on the other hand, is a structure very closely resembling the simple india-rubber apparatus used for the production of ether-spray and for other purposes. A central blood cistern—of which the following rough diagram may perhaps serve as a



tolerably intelligible illustration—has two openings, the one into the principal artery the other out of the principal vein. Across the former of these is

placed a valve admitting of free egress but cutting off all return. Across the latter is a similar apparatus acting in the inverse sense, permitting ingress and forbidding exit. The great vivisector's experiment clearly proved that when you tightly pinched the artery by which alone, owing to the valve at the mouth of the vein, the blood could escape,—the blood could not escape any longer. But that surely was not all? No. Not quite. Releasing the artery through which the blood runs out he applied a similar compression to the vein, by which alone, owing to the valve at the mouth of the artery, it could possibly run in. And then he found—that it did not run in!* And was as much delighted with the results of this “scientific” procedure as though he had clearly proved, by building a wall across the passage, that he could not get in to dinner when the dining-room door was shut.

Nor is this by any means the only instance of the kind. The majority of the so-called experiments are not strictly speaking experiments at all; mere repetitions for corroborative or elucidative purposes of the ordinary experiences of every day surgery.†

Let me give one more the result of which is simple error and in which the error is more unmistakeably with the experimenter and its origin even more clearly

* Page 54.

† As for instance with the famous exhibition at Court where he narrates how he dissected out and cut the internal jugular for the sake of showing that while but a few drops of blood escaped from the lesser orifice “a round torrent of blood” gushed down from the head; he quietly concludes his story with the remark—“You may observe the same any day in practising phlebotomy.”—Page 126.

and directly traceable to the method by which he worked.

"Having," says Harvey, "exposed an artery and divided it so that the blood shall flow out as fast and as freely as it is received you will scarcely perceive any pulse in that vessel. . . . An artery denuded and divided in the way I have indicated sustains no shock and therefore does not pulsate. *Whence it clearly appears that the arteries have no inherent pulsative power and that neither do they derive any from the heart.*"*

Now here we have the experimental method in its ideal form; experiment used not merely to illustrate conclusions previously arrived at by mental process but as basis for a conclusion of its own. And the conclusion is wrong.

Compare the simple cogency of the following piece of plain reasoning from the plain anatomical facts of the capacity of the left ventricle and the amount of blood it must as a mechanical necessity, expel at each contraction. "Let us assume" he says:—

"The quantity of blood which the left ventricle of the heart will contain when distended to be, say two ounces, three ounces, one ounce and a half—in the dead body I have found it to hold upwards of two ounces. Let us assume further how much less the heart will hold in the contracted than in the dilated state and how much blood it will project into the aorta upon each contraction—and all the world knows that with the systole something is always projected, a necessary consequence demonstrated in the third class, and obvious from the structure of the valves. And let us suppose as approaching the truth that the fourth or fifth or sixth or even the eighth part of its charge is thrown into the artery at each contraction. This would give either half-an-ounce or three

drachms, or one drachm of blood as propelled by the heart at each pulse into the aorta. Which quantity, by reason of the valves at the root of the vessel can by no means return into the ventricle. Now in the course of half-an-hour the heart will have made more than one thousand beats. Multiplying number of drachms propelled by the number of pulses we shall have either one thousand half ounces or one thousand times three drachms or a like proportional quantity of blood according to the amount which we assume as propelled with each stroke of the heart, sent from this organ into the artery. A larger quantity in everycase than is contained in the whole body!"*

And this as a matter of fact is, according to Harvey's own account the argument on which his great discovery is based and out of which it sprung. And a most clear cogent and unanswerable argument it is. The brilliant because simple synthesis of the long string of plain anatomical facts relating to the structure and capacity of heart, artery and vein. A piece of pure reasoning with which the experimental method of Vivisection has—happily—no more to do than the rites of Obeah or of Shaman.

I have already once before alluded to that dramatic scene on Carmel. Let me, in all reverence, draw from it now not an illustration merely but a suggestion. Let us divide between us Harvey's great treatise as the rival prophets of JEHOVAH and of Baal divided the bullocks of the decisive sacrifice. To you, gentlemen, as to the more numerous body I, like the solitary Tishbite, yield the choice. Take you the whole group of vivisectional experiments appealed to by the discoverer from his first page to his last. I will content myself with what is left—the plain and simple reasoning which could have been equally adduced, and would have been of equal cogency, had Mr. Reid's Bill been already

* Page 48.

passed, and the whole Executive of the Victoria Street Society present to enforce it. Then let the two be laid before any unbiassed arbiter trained to estimate the weight of evidence, and let him say from which of these two "methods of research" the great discovery sprang.

Finally, I may be told that the only discoverer is he who demonstrates. I accept the definition.

How then is this discovery, made as we have seen without the aid of Vivisection, to be most effectively demonstrated? Not certainly by experiment upon living animals. The essential, self-evident impossibility of any such demonstration has already been clearly shown. Is it needful that to you gentlemen, practically familiar as most, at all events, of you must be with the every-day routine of the hospital dissecting-room I should suggest a method which surely must suggest itself to anyone not absolutely a slave to the vicious tradition of Vivisection? You have but to take the first "subject" that comes to hand in any of the great teaching establishments at your disposal, and with the syringe which the porter will promptly furnish inject the aorta with matter of any colour or any series of colours you may prefer until the injection issue from the vena cava, and the thing is done.

Solvitur circulando.

One word in conclusion. I trust that nothing I have here said of Harvey or of his discovery will be construed into an indication of anything but sincere respect alike for the discovery and the discoverer. Having unfortunately—I would have avoided it if possible—

been compelled to mention the name of Servetus I am of course prepared to hear my present argument described as a 'simple assertion of plagiarism from him just as my former one was set down on similar grounds as taxing Hunter with copying Anel. It is not with any idea of preventing that assertion but simply to save time in refuting it that I conclude as I began, with a plain statement of what my views on that head really are. I think it most probable, then, that Harvey never saw or heard of that curious lucubration in the nebulous recesses of whose misty mysticism Michael Servetus has so oddly encysted the eminently practical deduction from his earlier experiences of the dissecting-room. I am very sure that, had it lain upon his study table from that day to this he would no more have been tempted to read it than my old friend Mr. Norman Lockyer* will be tempted to seek for information on the chemical constituents of the sun from the pages of the "New Dispensation." As practical possessions of medical science the lesser circulation is mainly, the greater entirely, the work of William Harvey.

And rarely indeed do we encounter a discoverer to whom the fullest credit of his work can be accorded with more ungrudging heartiness. It was no fault of his if, while his intellectual powers soared far beyond the ablest of his compeers his ethical standpoint yet remained, in some respects at least, only at their level. Two centuries and a half ago the moral responsibilities of power towards weakness were but loosely formulated as between man and man. The idea of claims on the part of any inferior class was an unrealised potentiality even of the Christian code.

* It may be as well perhaps to mention here that I am not accusing Harvey of plagiarising from Mr. Norman Lockyer.

And here Harvey was simply of his time. He cut open a dog with no more thought of cruelty and probably much less consciousness of wrong than, as a schoolboy he might have cut open his sister's doll. And the fault was the fault of his time. It was not for the brutality of his vivisections that his contemporary critics blamed him but for their "vain glory." And in all else—in singleness of purpose, in modesty of self-estimate, in moderation of tone even towards opponents whose claims to it at his hands were very small he sets to controversialists of every class an example which, in the last particular at least I confess that I for one find it sometimes easier to appreciate than to follow.

May I hope that in this last particular I shall find it followed by that great Association to whose Presidents and Council I have ventured most respectfully to address this humble contribution towards the advancement of medicine by research ?

CHAPTER VI.

THE ACTION OF POISONS.

AMONG the most specious of the fallacies of "Experimental Physiology" is that which represents the poisoning of animals as the only means of acquiring a "scientific" knowledge of the action of drugs on man. Let us examine a few representative cases.

Dr. Ringer's useful "Handbook of Therapeutics," p. 478, says, "Small doses of Opium excite tetanus in frogs; on the other hand, birds—namely, ducks, chickens, and pigeons—cannot be poisoned by crude opium, and Morphia Salts must be given in enormous doses." According to Leube (Reichert's *Archiv für Anatomie*, 1867, p. 630), it takes ten times as much Strychnia to kill chickens as other birds, weight for weight, and among mammals, the guinea pig is very insensitive to it. It has recently been asserted (*Boston Medical and Surgical Journal*, 1872) that on some monkeys it has very little influence.

Professor W. Rutherford, as we all know, is celebrated for his abominably cruel and very extended experiments on the biliary secretion of the dog. In Wood's *Therapeutics* (a work with a strong vivisection bias) the results of these experiments are thus described, page 446:—"Experimental evidence at present does not warrant positive deductions as to the effect of purgatives upon the biliary secretion of healthy dogs. The canine diet and digestion are so different from the human that it is to be expected that medicines

acting upon the digestive apparatus will influence dogs differently from man. Thus I have given doses of Elaterium that would have killed a man to some of the carnivora without causing the slightest purging. In view of these facts, the only fairly deducible conclusion in regard to the experimental evidence that has been brought forward is, that it must be received with the greatest reserve or be entirely laid aside when we desire to study the question as to the cholagogue action of remedies upon man, and that our conclusions are most safely based upon clinical evidence."

This is most valuable testimony as coming from one who announces on his title-page that he writes with especial reference to the application of the physiological action of drugs to clinical medicine. Quoting again from Dr. Ringer, *Therapeutics*, p. 454 (for of course on such a matter authority is everything, and we refer only to authors of the highest standing on their respective subjects), we come to Belladonna. He says, "certain animals like pigeons and rabbits appear to be almost insusceptible to its influence. It has very little effect on horses and donkeys." It is well known that Chloroform acts upon animals and man in a very different manner, and it is maintained by some authorities that it causes death in both classes by diverse means. Prussic Acid causes horrible convulsions in man, and most mammals before death. Frogs, however, are not convulsed by a poisonous dose.

Dr. Ringer (*Therapeutics*, p. 444) makes an abstract of Dr. Fraser's researches on the physiological action of the Calabar Bean—"he finds that this poison destroys birds most easily, while frogs require as much as will kill a dog."

We have it on the authority of Paul Guttman that

goats, sheep, and horses eat hemlock with impunity. Of Antimony (Woodman and Tidy, Handbook of Toxicology, p. 112), say, "it was long ago proved that large doses might be given to dogs, and that little effect was produced provided free vomiting occurred." Yet the writer of this article is so susceptible to its influence that quarter-grain doses produce in him the most utter prostration.

The injection of a small quantity of Ferric Chloride ($\text{Fe}_2 \text{Cl}_6$) into a nœvus has proved fatal in the human subject. Yet Grelin found that two drachms produced only vomiting in dogs and that twenty grains injected into the veins produced no effect whatever. Dr. Ogle states (*Medical Times*, May 4, 1867) "that a rabbit can tolerate enormous doses of Atropina administered either by the stomach or subcutaneously," whereas in the human subject its mere application to the skin has caused death in two hours (*British Medical Journal*, Jan. 21, 1865). The Carnivora and Herbivora are very differently affected by the Tobacco alkaloid (Woodman and Tidy, p. 428). Of Conia, Mr. Verigo (*Centralblatt*, No. 2, 1871) says "that in frogs we get paralysis, but no convulsions; but that in mammals convulsions occur after large doses and paralysis of the extremities only after small ones."

Of Ergot of Rye, Tidy and Woodman, say (p. 303), "rabbits are far less influenced by it than dogs are (1) because they are accustomed to a vegetable diet, and (2) because idiosyncrasy greatly modifies the action of narcotics in herbivorous animals." The same authors say that death has been caused in the human adult by eating fifteen Sumach berries, yet rabbits do not appear to be affected by them (p. 294).

Dr. Nunnely in his observations on the action of

digitaline on frogs (Ringer, Therapeutics, p. 407), says—
“ the physiological action of digitaline on the heart of the frogs would appear to be widely different from its therapeutical action in the dilated and weakened human heart in disease.” Let our thoughtful readers who have followed us thus far, reflect how they would like to be physicked with any of the above therapeutic agents whose actions on the lower animals had been ascertained without reference to their clinical behaviour on the human subject, and then say whether we have to thank the practical physiologist or the clinical observer for ascertaining for us the uses of the potent drugs enumerated above.

It may not be without interest to know that certain animals can eat with impunity what would poison us in minute quantities, but it is an audacious piece of impertinence and an insult to our common sense, to tell us that our doctors cannot write a prescription for our ailments till they have tested their materials on inferior creatures supposed to be like us.

CHAPTER VII.

THE BISHOP OF PETERBOROUGH
AND OVARIOTOMY.

WE have connected this important operation with the name of the Bishop of Peterborough rather than with that of Mr. Spencer Wells not because we wish to deprive Mr. Wells of his due position as an authority on the subject but on the contrary from a feeling that he has already been somewhat unfairly dealt with. To whom may properly belong the honourable position of premier operator in this special line is no doubt a more or less open question, and on the whole, perhaps, the esoteric opinion of the Profession—to be always carefully distinguished from that profound reverence for the fashionable Sir Oracle Windbag of the day which is of course *de rigueur* in any intercourse with mere outsiders—might incline in that respect towards Dr. Keith. But the question is one of some nicety; involving not merely the counting but the weighing of cases. And it is one in which, so far at all events as concerns the purpose of the present article, we have no especial interest. To have brought a troublesome and until recently thoroughly discredited operation to a pitch of something very like practical perfection is an achievement which may fairly be accredited to both. We are dealing with the subject not under its pleasanter aspect of a great surgical triumph but under its less favourable guise of an unusually discreditable vivisectional imposture. The discredit

of that imposture has hitherto been universally and exclusively attributed to Mr. Wells. It would seem that in this respect Mr. Wells has been, at least to some extent, wronged.

The physiological fiction in question is as follows. "A London medical man of the highest eminence," said the Bishop of Peterborough in the debate on the Vivisection Bill in the House of Peers on the 15th July, 1879, "owes a discovery by which he has saved hundreds of lives to a series of experiments performed upon a dozen rabbits." This discovery was subsequently explained to be the operation of Ovariectomy, and it has been pretty generally taken by the unsophisticated in such matters, as it no doubt was by the versatile prelate who started it, to mean firstly that Mr. Wells invented Ovariectomy, and secondly that its invention was due to his vivisectional experiments; the medical journals of course joining in the triumphant chorus that "with the new light gained by vivisection he (Mr. Spencer Wells) has thus saved the lives of between 500 and 600 women." Now fortunately in 1872 Mr. Wells had himself published a work, which has been not inaptly cited as a model of what a surgical work should be, dealing exclusively with the operation of Ovariectomy and giving the most carefully precise particulars of the 500 cases in question. Chapter 10 of this admirable work is devoted to "The Rise and Progress of Ovariectomy." And in this interesting history the operation, as applied to human patients, which according to the Bishop of Peterborough, the professional journals and other medical authorities of the day was a discovery of Professor Wells himself there first appears in an extract from the twelfth book of the *Δειπνοσοφιστῶν* of Athenæus. Taking a leap

over "some apocryphal records of later periods" Mr. Wells then proceeds to writers of the seventeenth and eighteenth centuries, as Vierus Riolan ("Opera prima," Paris, 1610; "Anatome," p. 142), Diemerbroeck ("Anatomia corporis humani," Lyon, 1679; l. I. c. xxiii.), Boerhave ("Prælect. Academ. in prop. inst." f. 5, pars 2 and 669), Graaf (De Mulierum Organ. Generat. inserv. Tract. nov." cap. 13), Plater ("Observ. libri tres," Basle, 1680, p. 248), &c., who either mention the operation in question as having been performed, or propose it for performance. So far however the operation was only one proposed to be performed upon the healthy subject and it is not till 1685 that Schorkoff expresses the belief that it would lead to a permanent cure of ovarian dropsy, "if the operation itself were less cruel and hazardous." Mr. Wells then goes on through various authorities too numerous to mention till he arrives at McDowell the American, to whom he justly and honestly ascribes "the undeniable merit of having been the first who guided by scientific principles enriched modern surgery with the operation." This being in the year 1808 whilst Mr. Wells' "own experience of Ovariotomy dates from December, 1857," will probably be accepted by any but an actual "experimental physiologist" as fairly conclusive upon the question of Mr. Wells's original invention of the operation; to which we really cannot see any ground for supposing that Mr. Wells himself ever laid claim.

Of the part played in it by vivisection, we can only say that in the whole of this very interesting historical chapter conducting us step by step from Galen to "Kimball, Peaslee, Marion Sims, and Storer," no solitary mention occurs of any single vivisectional experiment. It is not

till chapter xv., dealing with the "treatment of the pedicle"—or what to unprofessional readers we may call the "stalk"—of the excised tumour that we find the first reference to certain German vivisections, amongst which oddly enough appears one series perpetrated upon just the number of unhappy rabbits celebrated by the Bishop of Peterborough. And this is Mr. Wells' comment thereupon. If, says he, we could hope in human patients for the same series of changes as have been observed in healthy dogs and rabbits, we might agree more completely with the conclusions of the German experimenters. But it is one thing to remove this or that portion of intestine from a dog or a rabbit, and a very different thing to remove a large internal tumour from a human patient whose general health has been more or less affected by its growth, probably by repeated tapplings, and whose internal conditions have been greatly altered by the presence of the tumour or the adhesions which it has formed with surrounding parts. Wherein Mr. Wells puts the whole thing into a nutshell. In the simple phrase of Dr. Clay, vivisection had—and, as Mr. Wells has here so fully shown, could possibly have—"no more to do with Ovariectomy than the Pope of Rome."

Now, slightly modifying the words of the old song, "'Tis a pity when charming bishops talk of things that they don't"—so thoroughly appreciate as it might be desirable that they should before any *ex cathedra* pronouncement. And it was a very special pity that Mr. Wells should have been led by a mistaken, though no doubt chivalrous sense of obligation to recant the utterances of his own practical experience and sacrifice a hitherto unsullied surgical reputation in maintaining the

curiously anti-theological thesis of his episcopal patron. To sincere admirers, like ourselves, of Mr. Wells's operative skill, and of the perfect unreserve with which in his first admirable work he has placed the results of his large experience at the service of his less fortunate brother-operators it is painful to pass to the consideration of the efforts with which he has of late laboured to attribute his own legitimate successes to a source the utter futility of which no one can really recognise more fully than they have, as we have seen been recognised by himself.

So far then we have found nothing to attach the smallest blame to Mr. Wells. Very much otherwise. We find him not only emulating the most skilful of his brethren, and far surpassing all but one or two in the results achieved in a most difficult and delicate operation but setting them all the highest possible example in the frankness with which he lays before them for their profit the lessons of his own hardly-earned experience. Setting them also we are bound to admit an equally excellent example in the entire absence of assumption with which his own achievements are detailed and the heartiness with which all due honour is given to his predecessors, the real originators of the operation. As for the absurd pretensions put forward on his behalf by his self-elected episcopal patron, there is not a trace of them. In his own story of his own work, published with a pardonable pride in commemoration of his five-hundredth operation, Mr. Wells no more claims to have invented Ovariectomy than to have invented gunpowder. While, as we have seen, the only reference by Mr. Wells to that Ephesian Artemis, Vivisection, to whose merits the right rev. prelate bore such eloquent testimony as Divine Patroness of the invention, is a

plain, straightforward, common-sense repudiation of any possible bearing to be had by it upon the matter.

But then came the attempt to place some limit upon the reckless cruelty of men, not, like Mr. Wells, practical surgeons but simple experimenters whose only common ground with the surgeon was that they worked with the same tools, and with the same magic letters after their name. The word went forth that the profession must support its members of whatever type and every surgeon or doctor of reputation or notoriety was called upon to ransack his brain for the records of any vivisectional vagaries of his salad days and proclaim them to the world as the one only means by which his own reputation or notoriety had been achieved. To this call Mr. Wells, like so many others responded and with a promptness and a thoroughness which shows no small reliance either upon the firmness of his own position or upon the readiness of his hearers to swallow any statement however monstrous or absurd which should appear to tell in favour of the cruel quackery they were so anxious to support. At the completion of his five hundredth case Mr. Wells was not only unaware of having himself learned anything from vivisection but was strongly impressed with the impossibility of anything being so learned. A couple of hundred cases more appear suddenly to have convinced him that after all it was from vivisection that he had learned the most important point in the whole operation, and that vivisection itself was so inestimable a teacher that he had learned more by cutting up fourteen rabbits than he could ever have learned from the half-hundred times that number of human patients who had since passed under his scalpel !

The monstrous absurdity of the contention becomes

more monstrous still when we find what this all important point was, viz., the including of the peritoneum, or inner coat of the abdomen in the stitches by which the wound is finally closed. We have not space here to enter into scientific details, which to many of our readers would be as mystifying as they apparently were to the unlucky Bishop of Peterborough. We will confine ourselves to simply pointing out four plain facts :—

1. The treatment in question had been—as Mr. Wells knew—habitually and successfully practised upon human patients for nearly a score of years before his work began.

2. So far as some of the highest authorities upon Ovariectomy may be trusted the point itself is of comparatively but very slight importance.

3. Mr. Wells himself is not of this opinion. But the position it occupied in his estimate before the necessity arose of finding some point upon which to found a glorification of Vivisection may be judged by one very simple fact. Of 478 closely printed 8vo. pages in his book, written on the completion of his five hundredth operation, expressly for the instruction of his brother operators, and dealing most minutely with every successive step in the operation, this vital point, upon which as we are now to believe the whole success of the whole five hundred had turned, occupies—just three lines !

4. And finally, be the point itself important or otherwise, the difference as to liability to inflammation from the process in question between a rabbit and a human patient is notoriously—among physiologists at all events—so great that no deduction could have been arrived at from the cutting open and sewing up again of all the rabbits in Epping Forest.

So much for the wonderful experiments by which, at the cost of fourteen rabbits, Mr. Wells—alluding to Bishop Magee—invented the operation by which he alone had saved five hundred lives and added ten thousand years to the existence of the women of England! We cannot but regret that a professional man of Mr. Wells's unquestioned eminence should have lent the sanction of his name to so discreditable an imposition. For a fashionable bishop, bound to be "scientific" at any price, yet with barely time to pick up the evening's supply of science from the afternoon's hurried glance at the papers or the magazines, such a blunder is natural, if not excusable enough. But Mr. Wells knew what he was talking about; and though there is nothing in the fact of being a skilful operator which necessarily implies the adoption of a high moral standard we yet cannot but feel sorry to find the two qualifications so painfully divorced.

CHAPTER VIII.

PYE-SMITH AND WOOLSORTERS'
DISEASE.

It is now not very much more than two years since Dr. Pye-Smith, addressing that august body the British Association in glorification of his favourite cult, felt bound to produce for its edification something a little fresher than the well-worn Popliteal Aneurism or Circulation of the Blood. And certainly a "method of research" upon which is to be founded the whole future of medicine, would seem to require at least occasional endorsement from the work of some enquirer not already numbered among the more or less illustrious dead. So among the classes of unquestionable benefit to Therapeutic Science which as we know upon such high authority "bristle around us everywhere," Dr. Pye-Smith selected one at once so important and so incontrovertible as to place the virtues of vivisection beyond all reach of further dispute.

It is not every one who has heard of the complaint known as "Woolsorters' Disease." To the men of Bradford and its neighbourhood its name is well-known enough and there can be little doubt but that to the Yorkshireman of the North West Riding, any method of research which should be fairly shown to have provided for its successful treatment, would

come with an initial recommendation not easily to be superseded. So after passing over certain little experiments in the way of inoculation of cattle with the lightness of touch which, since the change began in the tone of public opinion on this subject, forms in the writings of the physiologists of to-day so striking a contrast with the bold outspokenness of Professor Ray Lankester and others only a very few years back, Dr. Pye-Smith proceeded to inform his audience how—"it has been discovered that an obscure form of disease, common among the wool-sorters of this country, is nothing else but this same splenic fever actually transmitted through the wool from the diseased sheep to the human being employed in the manufacture, and there, under very similar circumstances, the disease has been recognised as substantially the same as splenic fever in cattle. So the enquiry which at first sight was purely theoretical, and apparently of no practical benefit, afterwards appeared to be not only for the benefit of the cattle and sheep, and to the advantage of human property, but ultimately to be also of service in preventing this form of disease which affects the human beings themselves."

The "bid" was a bold one, and we have no doubt but that many of Dr. Pye-Smith's auditors went away both from that lecture and from the *réchauffé* of it subsequently delivered at Hull, convinced that "after all there was something to be said for vivisection." Perhaps some of them may be students of the *British Medical Journal*. To those who are not we commend the consideration of the following, from the issue of that paper of the 11th of last month.

"ON WOOLSORTERS' DISEASE. (By John Henry Dell, M.D., Bradford.)—During the investigation which has

been made into the cause and nature of this disease, the following facts have been educed.

“ First : It is not confined to wool-sorters, but affects other classes of workpeople who manipulate the wool or hair after it has been sorted. Fatal cases have been observed in packers, washers, preparers, carders, overlookers, and buyers. Until recently, it was not known that wool-sorters were more liable to have external anthrax than other persons. This form of the disease has also been met with in persons who have accidentally come into contact with infective material. A case has been reported of a boy having anthrax on the temple, from lying for a short time upon a bale of mohair ; and another, of a gentleman having anthrax on the right temple, from touching some ‘ Van ’ mohair with his right hand at the warehouse of a friend.

“ Second: Alpaca and mohair are not the only infective materials. Many fatal cases have occurred from the manipulation of other dry and dusty hairs and wools. ‘ Greasy ’ wools are less dangerous ; it is probable that the ‘ yolk ’ in them fixes the infective particles. All hairs and wools (excepting Algerian) may at times contain uncleansed fleeces from animals which have died from anthrax, and may occasionally communicate the disease to those who come in contact with them. I have seen such cases from the sorting of British wools, and have heard from my medical friends of others ; but the proof is not yet complete.

“ Third: The disease is not confined to this district ; fatal cases have been reported at Leicester, Norwich, Glasgow, Constantinople, and in Massachusetts. I think it will yet be found in other places ; and certainly in Peru and Asia Minor, and at the Cape.”

"As for treatment," observes the same journal in a leading article commenting at some length upon the three cases of this disease, all fatal, reported in its columns—"As for treatment, *we seem to be much in the same position as we were!*"

We subjoin a few more facts regarding the present position of "Science" with regard to this disease, the complete subjugation of which forms according to Dr. Pye-Smith and other enthusiastic vivisectors the latest and supremest triumph of their favourite art. According to the Annual Report of the Commission on Wool-sorters' Disease—

"The Society is aware that a Commission from the members and others was formed during the autumn of last year to investigate the whole question of Wool-sorters' Diseases, with a view to the discovery of the nature of the infective poison, and for the suggestion of the remedies best calculated to combat and prevent the disease. The Commission met eighteen times, and a considerable mass of information respecting cases, &c., has come before it. It must be said that the opinion of the members of the Commission is as yet divided as to the true nature of the poison producing so-called Woolsorters' Disease, though it is admitted by all that there is a virulent infective agent at work. Nor until a unanimous, or almost unanimous, conclusion can be arrived at respecting this is it the intention of the Commission to issue a decisive report upon the subject. Meanwhile the members have determined to pursue their labours till they realise the end in view. Many interesting points and collateral issues have arisen during the prosecution of the inquiry, not the least of which is the discovery that bacteria are present in other diseases beside that of wool-sorters, both in the

blood and tissues of human beings and animals, and if not before, at least immediately after, death; and, furthermore, that they are found in the fluids of healthy animals killed for the purpose of ascertaining this point, and that, too, so immediately after death as to lend the strongest probability to the opinion that they may exist in the healthy tissues during life; for in one instance they were found within half an hour after the animal was killed, and thus before decomposition could possibly have set in. Of course the great point for determination is whether a sufficiently marked distinction exists between what is called the bacterium bacillus anthracis and other bacteria; this point remains for decision, and there appears at present no reason to doubt that a satisfactory solution will be arrived at."

The above appears in the *Lancet* of 16th July. In the *British Medical Journal* of the same date Dr. Tibbits of Bradford writes:—

"There is much yet to learn concerning this disease. Moreover, our knowledge will not be likely to increase very rapidly, if we accept, in direct opposition to reason and the evidence of our senses, the popular dicta of ardent, but, unfortunately, misguided enthusiasts.

"But, to proceed. In the presence of a large number of medical men, Dr. Greenfield distinctly stated, in answer to a question I had previously put in the *Lancet* and *Journal*, that 'typical bacilli' meant bacilli typical of anthrax, but that he would not commit himself. What is the meaning of such language? Is any one the wiser for it? Am I right in venturing to consider it evasive, or, at any rate, ambiguous? The natural and just inference was, that he could not point out any character or characters, by virtue of which the so-called bacillus anthracis could be distinguished from any other.

If otherwise, why did he not give his medical brethren the benefit of such valuable information? In accordance with your correspondent's suggestion, and at the risk, I am afraid, of inevitably lengthening this epistle, I will endeavour to make my statements more complete by quoting authorities.

"Nägeli, a botanist of the first rank, and Brefeld, do not see any absolute necessity for dividing bacteria even into two distinct species.

"Cohn declares that hay-bacilli are identical, in form and size, with anthrax bacilli; and that the various stages in their development, including the spores, correspond in every particular—the only difference being the presence or absence of motility. That this distinction no longer exists, has been abundantly proved by Dr. Cosser Ewart and others.

"Messrs. Lewis and Cunningham have found innumerable bacilli in healthy rats and mice, about twenty hours after death, in every way morphologically identical with anthrax bacilli. Moreover, they have discovered that these bacilli vary greatly in size. Such a distinction, therefore, is absolutely valueless. The same observers have noticed large numbers of bacteria in dogs killed by the injection of chemical irritants.

"Dr. Burdon Sanderson, also, in experimenting upon a guinea-pig by the injection of boiled and cooled dilute solution of ammonia, made a similar observation; and he adds: 'Other chemical agents will lead to the same results, and always under conditions which preclude the possibility of any infecting matter from without.'

"Dr. Bastian, one of the highest authorities on microscopic organisms, says, regarding the doctrine of *contagium vivum*: 'Existing evidence seems to me abundantly sufficient for the rejection of this doctrine

as untrue.' And, as regards zymosis, he says: 'The all-important point is, not whether latent ferments exist, but whether causes, or sets of unhygienic conditions, can rouse or modify, in certain special modes, the activity of any of these myriads of potential ferments, of which the human organism is so largely composed.'

"Dr. B. W. Richardson is another strong opponent of the contagium vivum theory. Dr. Beale thinks the germs of the schizomycetes exist in all tissues; 'nor is the blood of man free from them.' M. Signol, of the French Academy, has corroborated Messrs. Lewis and Cunningham's observations.

"Leisering of Dresden states, that the bacilli found in splenic fever, and abdominal typhus of pigs, are identical. Other foreign authorities I forbear to mention, for want of space.

"Professor Greenfield states that, after death, before decomposition commences, the presence of bacilli, of the size and form of anthrax bacilli, without other bacteria, is distinctive; but, after decomposition sets in, the presence of similar bacilli, with other bacteria, is inconclusive. How is it possible to discover the precise moment at which decomposition commences? For, without this knowledge, the diagnosis, even after death, is impossible. But, apart from this, one of the illustrative cases related in the fourth Brown Lecture militates against the above-mentioned theory. Case 1 is said to contain motionless rods and a few ordinary bacteria; therefore, this is not a distinctive case of anthrax. But, further, he says that the only certain test is inoculation; and that this is negative when decomposition sets in. His inoculation experiments were nine in number, and in four only were any results obtained. What about the other five? Where is the certainty of

the test? An endeavour is made to explain it by taking into consideration the length of time which elapsed between death and the inoculation; but this will not carry conviction. Firstly, because the inoculation in Case 2 was unsuccessful, the *post mortem* examination being twenty-eight hours after death; secondly, because the inoculation in Case 6 was successful, the *post mortem* examination being twenty-six hours after death.

"Case 2 happened on May 21st, and Case 6 on August 29th. *Ceteris paribus*, decomposition would, in all probability, be more advanced in that occurring in very hot weather—viz., Case 6. Surely the temperature would more than compensate for the comparatively slight difference in the number of hours.

"By Brunell, Bouley, Bollinger, and others, it has been demonstrated that, in anthrax and septicæmia, the blood is just as poisonous when deprived of these organisms; and M. Bert has proved that the poison can be transmitted from animal to animal, with fatal results, without the appearance of any organism.

"The question put by Dr. Allbutt to Dr. Greenfield as to whether he considered these organisms to be the cause of the disease in question, was decidedly answered in the negative. Unless my ears deceived me, about this there could be no possible mistake. At any rate, this indicated the opinion of one well-recognised authority on the subject. As to the original idea being unquestionably correct, there is not, nor has there ever been, anything approaching to satisfactory proof of it. It was, and still is, a mere hypothesis, at present unsupported by trustworthy evidence.

"Personally, I have never yet ventured to express an opinion as to the true nature of Woolsorters' Disease. That, as yet, has not been demonstrated by anyone.

In the existence of some virulent blood-poison, which occasionally affects wool-sorters with a rapidly fatal disease, I firmly believe. On the other hand, I am as fully persuaded that the disease is not as prevalent as it has been represented to be. In the words of Professor Greenfield, I would say 'that the whole subject of anthrax is in a transitional state, and that the criteria hitherto accepted for its diagnosis are obviously inadequate.'

"There is much that is marvellous and fascinating in the study of these minute organisms. Its importance can scarcely be overestimated. At the same time, we must remember, that, in this wonderful world of ours, there are other and probably far more prolific sources of disease at work than these attractive little favourites of the day, yclept 'germs.'"

Which is highly satisfactory of course. If not particularly conclusive.

CHAPTER IX.

FERRIER AND LOCALIZATION.

A GERMAN periodical of high repute published at Stuttgart, *Das Ausland*, contained in Nos. 21 and 27 (June and July), 1874, a review by the editor, Friedrich Von Hellwald, of a work, "Materielle Grundlage des Seelenlebens," by R. R. Noel, which had been just published in Leipzig. In a second critical notice of this work, the editor of *Das Ausland* mentions Noel's reference to the experiments of Dr. Ferrier on the brains of living animals, and says in the text, "these experiments came from Drs. Hitzig and Fritsch." To this observation Herr Von Hellwald has added a long note, wherein he says further: "The first named of these searchers after truth has called my attention to the circumstance that everything of importance in Ferrier's discoveries has not originated from him, but has been fully detailed by himself and his friend, Dr. G. Fritsch, in Reichert's and Du Bois-Reymond's Archiv. In fact I have actually before me two separate issues from the *Berlin Weekly Clinical Journal* (1870, Nos. 19 and 43), of lectures by Dr. Hitzig 'on the electrical excitability of the Cerebrum,' and containing 'further investigations of the physiology of the brain confirming the truth of the statements.' In addition to these a considerable number of other writings by Dr. Hitzig have been collected in a work

just published by Hirschwald in Berlin: 'Investigations of the Brain.' As Dr. Hitzig has communicated to me, he found himself obliged to take this step in consequence of the immoderate system of robbery on the part of the London Professor (das alles Mass übersteigende Ausraubungssystem des Londoner Professors). Dr. Ferrier, to be sure, in his first publication just mentioned the names of the two German doctors, but he appropriated to himself the greater part of their discoveries, and besides placed the matter in such a light that their proceedings appeared to be extremely trifling and that they did not in the least know the value of what they had observed.*

"In the above-named work, which I have not yet seen, there is—Dr. Hitzig informs me by letter—a full exposition of these circumstances, and in dry words he has demanded of Dr. Ferrier to restore to him and Fritsch that which they had acquired with so much labour. English periodicals have taken no notice of Hitzig's reclamation; and, as he has been privately informed by friends, it has been pronounced to be impossible to allow an Englishman to be pilloried for such an affair by a foreigner in a English Journal. I can hardly believe in such party-spirit in English journalists, and therefore in the name of *Das Ausland*—which has always made it a duty free from all national Chauvenism to acknowledge the merits of foreigners under all circumstances—I have a right to raise my voice. I consequently request the representatives of British Scientific Journalism, particularly of the *Athenæum*

* This is so true, Mr. Noel informs us, that in writing (1874) my book in German and receiving publications from Germany for that purpose, I paid no regard to his mention of Hitzig and Fritsch when speaking of Ferrier's experiments.

and *Nature*, as colleagues, in the interest of scientific honor and truth, that they should cause a conscientious enquiry into the case I have stated to be made, the result of which naturally I do not here forestall."—THE EDITOR OF "DAS AUSLAND."

The appeal of the editor of *Das Ausland* to London colleagues met with a response in *Nature* (Vol. X., No. 249). In an article headed "Hitzig *versus* Ferrier," it is pointed out that the editor of *Das Ausland* had erred in supposing no notice had been taken in English journals of the work of Fritsch and Hitzig, for "*Nature* had been the first—or amongst the very first—to draw attention to the researches of the able German physiologists." Of four points of importance, resulting from the stimulation of the cortex of the brain by electricity, it is further stated, Dr. Ferrier can only claim one, whereas "he has retained for himself the whole credit for the other three." It is said too, that Ferrier in the *London Medical Record* had expressed regret that he had not indicated some *minor* (*sic*) coincidences between his observations and those of Fritsch and Hitzig. "Nevertheless," it is added, "Ferrier still seems to fail to realise that his true relationship to the original discoverers of the method he employs is that of disciple to master, and not that of an equal as far as the subject itself is concerned. And to show that due credit has not been given in the right direction, it may be mentioned that in this country the localisation of the cerebral functions has thus become associated with the name of Dr. Ferrier so much so that Dr. Carpenter in his recent work on Mental Physiology has a chapter on the subject in which the names of Fritsch and Hitzig are not even mentioned, the heading being 'Dr. Ferrier on the brain.'"

The foregoing extracts from *Nature*, though sufficing to show that Ferrier had not done justice to the German doctors, nevertheless, do not embrace all the proofs of his disingenuousness—to use a mild term—which Dr. Hitzig specifies in his work, “Investigations of the Brain” (Berlin, 1874), which, as he mentions in his letter to Dr. V. Hellwald, he had been obliged to publish in consequence of the “immoderate system of robbery” on the part of the London Professor. This work, which we have carefully read, appears to us fully to confirm the truth of all that Dr. Hitzig has said. The first section in it is a reprint of the original article by Fritsch and Hitzig in Reichert’s and Du Bois-Reymond’s *Archiv.*, 1870; the second is also a reprint from that periodical headed, “Researches in the Physiology of the Cerebrum,” 1873.

In the third section, however, “Critical and Experimental Researches in the Physiology of the Brain in connection with the Researches of Professor Ferrier in London,” those facts are enumerated on which the foregoing estimation of Dr. Ferrier’s proceedings are founded, and it is also shown that his method of employing exclusively the inductive current, does not and cannot, according to the experiences of the German physiologists, give reliable results. And in respect to Ferrier’s assertion that Fritsch and Hitzig had not employed the induction current to any extent, they reply: “If we have used the induction current less often than the galvanic, nevertheless we have certainly used it as much as Ferrier, and quite enough to convince ourselves of its uselessness for establishing the centres for movements Ferrier claims to have done.”

To follow Dr. Hitzig in all he says respecting Dr. Ferrier’s proceedings, his inexactness, and his

erroneous deductions, &c. ; and, on the other hand, to state the results of Fritsch's and Hitzig's experiments with galvanism on the brains of dogs and other animals, would be out of place here, and moreover painful to our readers. We pass, therefore, to another section of Dr. Hitzig's volume, headed: "Dr. Ferrier's conclusions" (p. 109), and quote from it a paragraph printed in larger type than the rest of the text: "To recapitulate in few words," it is said, "what Ferrier by a method so open to objections has, in a superficial way accomplished, we may say that by means of strong electric currents applied to the frontal and basilar parts of the brains of dogs and cats movements as of eating (*Fressbewegungen*) can be produced. Herein consists his merit. On the other hand, he has not with any certainty again found the points of irritation which we have noted; he has made a number of statements relating to the effects of irritation, resting on varying or erroneous methods; and lastly has adorned his work with discoveries—without mention of names—which do not belong to him, but to us."

CHAPTER X.

PASTEUR AND ANTHRAX.

THERE is no argument upon which the vivisectionists have relied so confidently, or which they have so persistently brought forward upon every possible occasion as the alleged triumphant success of the inoculation experiments of M. Pasteur and the immense benefits they were (going to) confer upon cattle and their owners. We purpose, when occasion shall serve, to deal thoroughly with this, perhaps the most practically mischievous of all the delusions of the modern pseudo-science of Physiology. For the moment we would direct attention to the Report of the Commission appointed by the Hungarian Government to inquire into a procedure in which as presiding over the largest pastoral country of Europe they are more especially and personally interested.

“We cannot,” says that report, “overlook the fact that after the protective inoculation the deaths from other diseases, or, more correctly, those in which the post-mortem appearances were those of other diseases—catarrh, pneumonia, distoma, strongylus, and pericarditis—and not those of anthrax, occurred exclusively *amongst the inoculated animals*. It follows from this that the fatal issue of other severe, but latent diseases, is accelerated by a protective inoculation.” Starting from these indisputable premises the report goes on to observe that from a practical point of view,

it is pretty nearly the same whether the loss is caused by anthrax or other diseases, and that therefore deaths from the diseases induced by the so-called "protective" process should be added to those from anthrax, all of which it must be admitted is rather far from affording unmixed encouragement, even so far as regards the simple question of protection from contagion, and that conveyed by direct inoculation. When we come to take a somewhat wider view the result is less satisfactory still. "If we do this," it goes on to say, "and add together all the deaths which occurred after the protective inoculation and the control infection, we get, as the chief result, that of the uninoculated animals 94 per cent., and of the inoculated 14.5 per cent. died. Even thus a very considerable difference remains, but nevertheless the mortality of the inoculated animals, 14.5 per cent., is by no means small. This mortality rate corresponded so closely in two experiments, 14.78 per cent. and 14.27 per cent., that it almost appears to be a rule, and therefore we must not assume that experiments carried out on a larger scale would yield a more favourable result. It must not be forgotten, too, that we have here to do with average numbers, while in practice we have to deal with single cases, and in such cases the mortality due to the protective inoculation mounted, for anthrax alone, as the preceding experiment showed, from 3.5 per cent. to 10 per cent. of the inoculated animals. When we consider all these facts, the fear seems not unfounded that in practice a still higher rate of mortality would be reached. On the other hand, from a practical point of view, we must set against this mortality, not the experimental mortality of 94 per cent. of the uninoculated animals, but only the rate of

mortality which occurs among the cattle in districts affected with anthrax, and which is very considerably less."

"There are still several doubts," says the report, "about the method from a public health point of view. The doubt that the protective power is only exerted for a certain length of time is of minor importance, for if we only succeed in removing the other difficulties, the protective inoculations can be so comparatively easily and cheaply carried out, that in case of necessity they could be repeated every year. Of great importance, however, is the question whether the meat, milk, &c., of inoculated animals can convey anthrax to man; and certainly before the protective inoculation becomes general, the question must be solved as to how long a time must elapse before the flesh and milk of inoculated animals could be allowed to be used as food. When we consider that the inoculative material contains anthrax microzymes in colossal quantities, although of diminished virulence, and that the microzymes multiply to a gigantic extent in the organism of the inoculated animals, we see that the general employment of protective inoculations would spread these microzymes in inconceivable quantities through the whole country. Deaths will occur at all times, even among the inoculated animals, from other diseases, although fewer from anthrax. The possibility is not excluded that the microzymes which would be liberated from the dead bodies when they became scattered might regain, in some way or another, their original virulence, and that, despite all trouble and cost, they might in this round-about way affect men and other animals. This is all the more to be feared, as the

carelessness with which people even now treat the bodies of animals which have died from anthrax, would then be as much as possible increased by the belief in the omnipotence of the protective inoculations."

The conclusion at which the committee of enquiry arrives is a little complicated by the reporter's extreme desire not to appear to run too directly counter to the popular "scientific" craze of the day. A cynical student of human weakness in search of a master-example of the combination of the *maximum* of conviction with the *minimum* of courage could hardly wish for anything better than the following truly Hibernian conclusion of Dr. Aladár von Rószahegyí.

"Everything considered," he tells us, "I entirely concur in the opinion of the committee, that the immediate general application of Pasteur's method in the form demonstrated to us here would be precipitate; that it should, least of all, be recommended and disseminated under the authority of the State; and that, with regard to the other possible sanitary evils, the performance of protective inoculation by private individuals on their own account should be completely forbidden, and only allowed under the condition that such operations should be performed by a Government official acquainted with the subject."

A mode of procedure which should be "completely forbidden" to private individuals and "least of all recommended or disseminated under the authority of the State," is a discovery over which "Experimental Physiology" may possibly exult, but which would hardly present itself in the light of a triumph to the disciples of any less "scientific" cult.

CHAPTER XI.

CEREBRAL LOCALIZATIONS.

THE doctrine of cerebral localizations so triumphantly put forward as a result of vivisection proved to be of the greatest benefit to mankind, when it is considered closely, is far from substantiating the claims made on its behalf. Originally one with phrenology, and intimately interwoven with Gall's fanciful theory of twenty-seven special cerebral organs for an equal number of special faculties, as indicated by well-marked prominences or regional development of the skull, taking no account of its irregularities of growth or of the unequal thickness of the various constituent parts, cerebral localization entered into a new phase with Flourens, who demonstrated, to the satisfaction of his scientific brethren, that in the outside grey matter of the brain there are no special centres, and that in proportion as you slice away brain substance, you deprive an animal of its intelligence, and reduce it to the condition of a machine. The conclusions thus arrived at were extended unreservedly to man. It is true that most of Flourens's experiments were performed on pigeons, and that there are some differences between the brain of a pigeon and the brain of a man ; but on these the physiologists of that day touched lightly, and they were content to accept the data afforded by vivisection as equally applicable to pigeons and to their own species.

Experiments of this nature were continued by many others, among whom were Majendie and some of his pupils, all tending to prove that the substance of the

convolutions is not sensitive, and that it contains no special centres for localization of function. In 1870 Hitzig and Fritsch opened up a new era of experimentation, by announcing that what was supposed to have been clearly demonstrated on living animals by their predecessors, was all a mistake, that some parts of the surface of the brain were sensitive to the stimulus of electricity, and that when certain regions were excited, certain movements followed as a matter of course. They were also of opinion that in other regions of the brain surface electrical stimulation produced sensations. Thereupon great excitement ensued in physiological circles, and fresh hecatombs of animals were sacrificed to prove the old theory false and the new theory true. In Hitzig's steps followed Ferrier, the notorious English physiologist, with his barbarous extirpations of portions of brain in dogs and monkeys, and his electrical stimulation (the so-called Ferrierization) of other parts, which have led him to maintain as distinctly localized centres in the convolutions for certain definite muscular movements, as any of the organs presiding over special faculties contended for by Gall or his followers. In the same path, not to mention a score of other vivisectors less well known to the English public, followed also Goltz with his washing and syringing out of the brain substance of dogs which must have been more or less pets of their former owners, for we are told by himself that he directed his laboratory attendant to buy for him preferentially dogs which had been taught to beg. The greater number of these wretched mutilated animals died, as he tells us himself, of inflammation of the brain, the result of his manipulations; and it will perhaps be remembered by our readers that a

horrible death befell the ill-fated Mary Rafferty, a Ferrierized hospital victim of modern theories of cerebral localizations.*

It might perhaps have been expected that a certain uniformity of sentiment would prevail among physiologists in regard to the cerebral localizations which it has cost so much animal torture to determine. The contrary is however the case, as the discussion on the subject between Goltz and Ferrier at the late International Medical Congress sufficiently demonstrates. We take the following from Goltz's address on the Localization of Function in the cortex cerebri: "Flourens thought that all parts of the cerebrum subserved the same functions. Fritsch and Hitzig considered it to be proved that the cortex of the cerebrum (the convolutions) may be laid out in circumscribed regions or centres, each of which presides over a special function. The investigations of Fritsch and Hitzig were continued first by Ferrier, and afterwards by a number of other experimenters. I will not here enter into the details of their observations, because I only wish to insist on the most important differences between them. The hypotheses of all these investigators have so much in common, that they divide the cortex of the brain into small circumscribed spaces, centres, or spheres, to each of which they ascribe a particular function. In the limitation and position of these spaces, and in the functions attributed to them, we find the most remarkable contradictions between different experimenters, and especially between Ferrier and Munk."

* *American Journal of the Medical Sciences*. No. CXXXIV. April, 1874, p. 308. The Volume may be seen at the Offices of the Victoria Street Society for the Protection of Animals from Vivisection, 1, Victoria Street, S.W.

"Notwithstanding these contradictions, the medical public accepted with acclamation these modern doctrines of localization. The writers of a number of new manuals waxed enthusiastic over them, whereas some hesitation should have been felt in accusing so clear-headed a man as Flourens of such glaring errors of observation. The reason that the new doctrines were so credulously received was evidently that the ground was ready for them, and that pathological observation had led up to the need of separation of the functions of the brain. There was a fascination in the precision of the new teaching, and in the apparent agreement between the results of stimulation and destruction. *A fruit may, however, appear very tempting, and yet be quite worm-eaten at the core, and it is not difficult to show the worm-eaten place in all the localization hypotheses that have yet been put forward.*" (The italics are our own.)

Into the details of his argument it would lead us too far to follow Professor Goltz ; but the above quotation may suffice to show how shaky is the edifice constructed on so much animal torture. In regard to Ferrier, especially, Goltz expresses himself very strongly of opinion that there are no sufficient grounds for many of his localizations, and that he has mistaken in many instances the immediate results of a destructive experiment for the permanent loss of function entailed by the loss of an imaginary centre in the cortex of the brain. To this Ferrier replies at length,* the gist

* That is to say, it is Ferrier who makes this reply, *according to the published statements*. It will of course be understood that we simply take those statements as they stand without making ourselves in any way responsible for their truth. Indeed, should any unpleasantness arise, we should fully expect to find that they, like those of the *Lancet*, &c., in the notorious Bow Street case had only

of his reply being that, however it may be with dogs, in the monkey and man it is different; and he goes on to enlarge on experiments on monkeys lately performed, finishing up with the demonstration of a monkey he had kept alive for seven months without about a quarter of the surface of its brain. The inference is very clear, that future experiments, to be of value, must be performed on animals highest in the scale, *i.e.*, monkeys and men. In this connexion the proposal latterly made by an Englishman "ardently interested in the pursuit of vivisection," in a letter which may be seen on application at 1, Victoria Street, is worthy of consideration. He says: "I would propose to hand over all murderers sentenced to death—not to the hangman, whereby their immolation becomes of no service to society, but to the scientist—the accredited and licensed vivisector, that by becoming his subject, they might expiate their crimes in adding to our knowledge." Such a proceeding would doubtless meet with the approval of some eminent physiologists, one of whom, Brown-Séquard, has no word of blame, in reporting the case, for certain doctors who in the present century vivisected a criminal after he had escaped with life the last penalty of the law.

Goltz refers us to pathological observation, and taking the hint, we turn to clinical records, and to the opinions of some of the great professors of clinical medicine, themselves not unlearned in the latest teachings of science. In an important lecture delivered shortly after Brown-Séquard's renunciation of cerebral localizations in the *Lancet*, in 1876, had created quite a sensa-

been "accurate enough for scientific purposes" and that the experiments had really been the work of—well, let us say Professors Hitsch and Fritzg.

tion in the medical world, Prof. Charcot enumerates the members of what he calls the scientific trinity, on which correct knowledge of the functions of the brain depends. These are:—1. Normal, human, or comparative anatomy; 2. Physiological experimentation; 3. Clinical observation, aided by methodical and minute examination of organic lesions. In the course of this lecture Charcot remarks:—"I cannot conceal from you that, to my mind, the information furnished by the last group, constant comparison of pathological anatomy with clinical observation, must always figure as the most important and decisive; for if the former may lead us on in the path of localization, the latter alone allows us, as far as regards man, to form a definitive judgment and to supply proof. We must not forget that it is man we are considering—man, who relatively to the functions of his higher nervous centres is so far removed, in many points, from even animals highest in the scale. In what concerns him, in connexion with our subject (cerebral localizations), the results of the most ingenious and best conducted experimentation can only furnish presumptions more or less well founded, but no absolute demonstration. I repeat then, that it is in man himself that proof must be sought."

"It appears at first sight that observations limited to the human domain, and deprived of the powerful lever of experimentation, are condemned to play a subordinate part. But this is an appearance merely, by which we must not allow ourselves to be deceived. As it has long since been remarked, the conditions of an experiment, produced, indeed, spontaneously, are realized daily in man under pathological circumstances. In order to make use of these pathological experiments, one must

only learn to bend to the necessities of a situation which is undeniably very different in many respects from that which deliberate experimentation on an animal brings about, but which is not always more complex. If it is true that observations made on a sick man by the light of physiology demand in general more time, more patience, than the corresponding studies carried out on a vivisected animal; if it is true that in man the conditions of the phenomena, contrary to what occurs in the laboratory, can neither be modified nor reproduced at the will of the observer, it is equally true that the malady often determines in the body of man, lesions more exactly limited to one organ, to one tissue, more systematic so to speak, and more compatible with the persistence of life, and the integrity of functions not directly interested, and, consequently, more favourable to methodical and prolonged analysis than are the mutilations produced in an animal by even the most skilful physiologist."

It follows from all these considerations that our definite knowledge of the functions of the convolutions of the brain is still very meagre. And, moreover, as a striking illustration of what we owe in this matter to clinical observation and pathological research, as distinct from vivisection, what we do know best is that there is a region (Broca's convolution), on the left side, connected with the localization of speech, and that there is good reason to believe that the corresponding region on the right side is sometimes, at any rate, active in supplying the nervous influence when the left hemisphere of the brain is injured and Broca's convolution destroyed. It is not a little curious that this knowledge, which could not have been acquired by vivisection (other than vivisection of man), should be the best ascertained of

any we possess relative to the localization of functions in the brain, and it suggests the reflection that where there are necessarily so many fallacies involved in the application of facts derived from the vivisection of animals to the human brain, the most scientific—not to say the most humane—method of investigation of cerebral localizations, with any view to the benefit of the human race, must be that of clinical and pathological research.

CHAPTER XII.

THE LAMSON IMPOSTURE.

"PARTURIENT MOUNTAIN" probably never yet brought forth more distinctively "muscular abortion" than in the monstrous exhibition of physiological hocus-pocus in connection with the notorious murder case now happily brought to a satisfactory termination.

We are not, of course, speaking of that portion of the scientific evidence which consisted in the application to the lips, tongues, &c., of the experts themselves, of extracts from the viscera, &c., of the victim, with the result of distinctly producing in almost every instance the well known and easily recognized phenomena produced by the similar application of aconitine. With one partial qualification these tests were thoroughly satisfactory. It might possibly be objected that the imagination is apt to play strange tricks and that the somewhat delicate results of "tingling numbness," &c., would have been more conclusive if produced in the tongues and lips of persons not primed beforehand with full knowledge of the drug sought for and the precise signs by which its presence was to be recognised. We would at all events suggest that in any future testings of the kind the extracts should be made by one person and the sensations produced by them recorded by another, who should not know beforehand what it was that he was expected to taste or feel. In the present instance however some at least of the sensations produced seem to have been of sufficient strength pretty fairly to obviate this objection. A

smarting and tingling lasting for several hours could hardly have had its origin in the imagination. And if real they are conclusive. They are precisely the effects produced upon lip, tongue, &c., by aconitine—and by nothing else. If therefore they were produced here the conclusion was irresistible as to the presence of aconitine in that which produced them.

We shall perhaps be told—a physiologist would almost certainly tell us—that the accuracy of these tests has been placed beyond a doubt by the confession of the wretched man himself. This is indeed an almost typical example of the kind of logic upon which at least ninety-nine in every hundred of what the vivisector is pleased to term his “conclusions” are based. To a healthily constituted intellect it is hardly necessary to point out that Dr. Lamson’s confession has no bearing whatever upon the subject. The case was clearly proved altogether independently of any scientific evidence; and the proof was all before the analysts throughout their investigation. Furnished with a similar clue any gipsy from the nearest common, any advertising fortune-teller, furnishing the maids-of-all-work of Stepney or Clerkenwell with wealthy and titled sweethearts at half-a-crown apiece, would for an equal fee have evolved from the same well thumbed pack a precisely similar result. But we should hardly accept the prophecy as conclusive evidence of the occult influence of the heavenly bodies or the magic virtues of the ace of spades. The justification of these physiological as of all other arguments must be found, if anywhere, not in the, possibly accidental, tallying of their results with a thoroughly foregone conclusion, but in their own plain accordance with the recognised rules of reasoning and common sense.

It is remarkable that while these requirements are thoroughly complied with in the experiments to which we have so far referred and which form one section of the researches on which the "scientific" evidence in the case is built up, in the other section—that of the vivisectional experiments—they are just as thoroughly disregarded. The test of taste and sensation was conclusive of the presence of aconitine, not because aconitine will produce those sensations but because they are thus produced by nothing else. A chop and a pint of stout make a good dinner. But it is quite possible to dine well without either of those condiments. A flash of lightning will knock a man down. But when Policeman X finds Patrick Donovan on his back in the Seven Dials, with a fine pair of black eyes and half his teeth down his throat, he does not immediately write off to the Meteorological Department to report a thunderstorm. But then Policeman X is not an experimental physiologist. For precisely this,—neither more or less—is the argument gravely tendered by Messrs. Stevenson and Dupré from their experiments upon the unfortunate mice, subjected at their hands to a painful and more or less lingering death, nominally in the interests of justice; really, it is to be feared with the simple object of making a little capital for the sorely discredited cause of the vivisectionist. Aconitine, we are told if injected under the skin of animals will, if strong enough, produce death. No doubt it will. So would any one of half-a-dozen or more alkaloids beside. So would a score of other things, not alkaloids nor in any way like them. So would, according to M. Vulpian, the simple, perfectly healthy saliva of the operator himself, of which he has probably in the course of his life swallowed harmlessly enough to

drown many mice, but a very few drops of which injected in precisely similar fashion to that employed in the case of the extract from the viscera of poor Percy John would have had a precisely similar result.

And this is literally all that vivisection was able to do towards corroborating the evidence of a crime of just the very class in which its boasted aid would have the most value and should have been most effectively applicable. To call such trifling puerile would be to libel most unjustifiably the intellectual capacity of the average Sunday-school boy. We will not credit Messrs Stevenson and Dupré with so cynical a contempt for the sound judgment of the jury box as to have brought it forward with the idea of influencing in any way the verdict in the case of Dr. Lamson. The real object with which it was adduced is tolerably transparent. These "highly scientific" experiments were meant to bear not upon the Old Bailey case of "Regina v. Lamson," but upon that of "Humanity v. Scientism" before the great court of the Public Conscience. Two eminent scientists, we shall be told, witnessed the death of half a score or so of mice, and gave solemn evidence of their procedure. And Dr. Lamson was convicted. *Argal*—But we need not go further. Any one who knows anything of vivisectionist logic will be able to draw for himself from such vivisectionist premises as these the inevitable vivisectionist conclusion. Let us hope the time may come when a too humble-minded awe of anything that parades itself before us in the lion's hide of self-styled science shall no longer, even to the most diffident, invest with the seeming weight of a profound philosophy the simple thistle-song of the Experimental Physiologist.

CHAPTER XIII.

TUBERCULOSIS.

THE medical mind appears to be periodically turned by some scientific craze or other, and to be, when under its influence, incapable of exercising sound discrimination and common sense. For many years it has been considered rank heresy to refuse to pin one's faith to carbolic acid and all the details of Lister's method of treatment, and the claims put forward on its behalf to the eternal gratitude of mankind have been simply astounding. Signs of reaction are, however, beginning to show themselves, and there are not wanting even men of eminence who decline to recognise in Listerism the *ultima Thule* of surgical practice. It may be that this is but an indication of the approaching end of one craze and the advent of another, for as such we cannot but consider the almost unanimous acceptance of the new theory of consumption or tubercular disease.

Koch's experiments, made known to the English lay public by Professor Tyndall with a loud flourish of trumpets, are interesting in more ways than one. They confirm the experience of many a careful clinical observer, that the healthy may not with impunity live in intimate association with the diseased, and that tubercle may be with no great difficulty developed in the weakly and the young, by confining them for the greater part of the twenty-four hours in close, ill-aired

rooms, contaminated with exhalations from consumptive patients. If an individual be capable of developing tubercle at all, he has little likelihood of escaping when placed in such conditions that he is forced to breathe air which has passed many times through a tuberculous lung, and all analogy points to material particles proceeding from the lungs or other affected organs as the carriers of infection. So far Koch's results accord with sound reasoning and established facts. Before however putting any faith in the triumphant announcement made recently in the *Times* on the subject of tubercle, let us remember that the man who makes it is the same Professor Tyndall who, a few years ago, announced in the same manner in the *Times* that Dr. Klein had just discovered the germs causing typhoid fever in the pig, and who prophesied with equal temerity that that disease would soon be under complete control. The sequel to the story is that after Klein's vaunted researches had been published in the Royal Society's Transactions, together with a plate of the morbid organisms, it was found out that these organisms consisted only of albumen coagulated by alcohol, and Dr. Klein was compelled, by the exposure of the deceptive nature of his "germs," afterwards to publish in the Transactions a recantation and withdrawal of all he had previously published and Tyndall trumpeted.

Experientia docet.

The fact is that our exact knowledge of the natural history of tubercle is still meagre. The discussions at the International Medical Congress of 1881 on Tubercle and on Micro-organisms show clearly enough what discrepancies exist in the minds of eminent scientific men of all nations. Virchow expressed it as his opinion

that "the secondary eruption (of tuberculosis) might be produced by a noxious substance—he would prefer not to say (with Dr. Creighton) by a *virus*—but this need not come from without. It might be produced in the body itself by a process of auto-infection. He thought that by a multitude of processes a substance might be produced that could infect the neighbouring tissue, also the whole body, and produce the tubercular eruption. He differed from Klebs and Creighton in not thinking it necessary to assume the existence of one special virus. . . . Experiments on herbivorous animals were somewhat fallacious—first, because their natural food was so different from that given them experimentally, and then because of the enormous frequency of tubercular disease in these animals. Experiments should be made on animals which resembled man in their food." Virchow here spoke of feeding, not of inoculation, experiments; but the remarks on the enormous frequency of tubercle in herbivorous animals are as applicable to one as the other, and it ought to be borne in mind that Koch's experiments were made on herbivorous animals—guinea-pigs and rabbits.

"Béchamp called attention to the part played by microzymes in the production of pulmonary tubercle in the calcified state. He thinks that in the lung the molecular granules break up and destroy the cell and remain, becoming impregnated with carbonate and phosphate of lime. These molecular granules or microzymes appear there under the form of minute spheres, often joined together in the form of a figure of eight. These granules put into starch paste, multiply, and give rise to fermentation, with the separation of hydrogen and carbonic acid, and the formation of

butyric acid. At the same time, the microzymes develop and become true bacteria."

Lister and Cheyne denied that micro-organisms are present among the tissues in the healthy state of the animal body, and Cheyne's theory was that in certain disordered states of the system micro-organisms were not immediately destroyed in the body, as in the healthy state, but could live for some time. Bastian contended that such organisms may arise in the body either where local conditions are favourable or where the general constitutional state of the patient is much impaired or disturbed.

"Dr. W. Roberts suggested that these minute organisms were an entire sub-kingdom in the animal world, which, though undistinguishable to us for the most part in form, had distinct vital properties and phenomena. Applying to them Darwin's theory of evolution, there was nothing strange in the idea that the ordinary organisms formed in wounds might change their normal character and become infective, leading to blood-poisoning in its various forms. By evolutionists time was not measured by years but by generations. It was said that bacteria doubled themselves in twenty minutes, and that in a fortnight they had a thousand successive generations, which was equivalent to a thousand years in the life of the wheat plant and 30,000 in the life of man."

Klebs includes bacteria and micrococci in the vegetable group of Schistomycetes, and places tuberculosis in the group of infective tumours. He pointed out how extremely difficult it is to demonstrate the presence of

micro-organisms in such structures as tubercles, and added that even the staining method (Koch's) "has till now given us no trustworthy results." This from one who is an acknowledged partizan of the new theory of tubercular infection by means of parasitic micro-organisms, is an important admission.

Hüter was of opinion that the micro-organisms active in the production of infective diseases are not absolutely specific, but will according to varying circumstances produce different diseases.

Fokker was the ardent defender at the Congress of the chemical theory of infection. According to his view, supported by the experiments of Davaine, Panum, and others, a virus is reproduced in the body by means of simple organisms, which exist there normally, and which only become a source of danger after they have combined with a soluble substance introduced from without. He concluded by expressing the hope that he had convinced his colleagues of the non-existence of specific bacteria, and that they would all admit that Nägeli's hypothesis, paradoxical as it might appear, was far more probable than the opinion of his antagonists, who had adopted a new species for every function exercised by the Schizomycetes. "Une seule espèce, Messieurs, mais l'adaptation !"

Thus conflicting as were the opinions in regard to the part played by micro-organisms in the body in 1881, Koch's announcement of his tubercle bacillus, and its infallible power of inoculation and reproduction after successive cultivations, has been received with an enthusiastic fervour of belief which makes one question

whether this century has been rightly designated as one of scepticism and not as one of faith. Only yesterday as it were, all was uncertainty and conflicting testimony on the question of tubercle in the great cosmopolitan gathering of scientists. To-day the secrets of consumption have been wrested from nature by the sacrifice of a few guinea-pigs and rabbits, and doubt is no longer admissible. Only last year animals of the same class which Koch has infected with his tubercle bacillus, and which Virchow informs us suffer from tubercular disease with enormous frequency, were, according to an article published in one of the best Paris medical journals, rendered tubercular, in varying degrees, by the injection of various irritating or indifferent substances, such as Spanish fly or powdered lycopodium, and the demonstration was considered to be complete that tubercles were developed in consequence of mere mechanical obstruction to the circulation, with or without chemical irritation.

The sober facts of the case, shorn of declamation and of rhetorical ornament, are simply these: Koch's experiments are scientifically interesting, but they are not conclusive, and they are capable of different interpretations. When the first rush of novelty is over, we may confidently predict that his experiments will be considered less convincing, and that counter experiments will be held to prove something different, if not contrary, to the results we are so confidently asked to rejoice in now. Even these results are as usual, not that tubercle may be cured—the demonstration of the curability of many forms of consumption has proceeded from clinical observers, not from experimenters on animals—but that tubercle may be conveyed artificially

to a healthy animal and presumably therefore to man. Interesting as such a fact may be to the scientific investigator, it is less interesting to the general public, which craves for cure and not for the perpetuation and spread of so painful and fatal a disease.

In conclusion, and in proof that the English press is not quite of one mind, we give the following letter from a medical contemporary, *The Medical Press and Circular*, for May 13th:—

“SIR,—I am glad to see that you have had the courage to call in question Koch and Tyndall's conclusions respecting tubercular disease. I say *have had the courage*, for it seems to me that the scientists of the day are absurdly dogmatic, and so ‘puffed up’ with the *aura popularis* as to have become quite impatient of contradiction. Permit me to supplement your articles on this subject with one or two thoughts which have occurred to me since reading Professor Tyndall's letter in the *Times*. It has long been known that certain bacteria, vibrios, etc., find a proper *nisus* in the degenerations of tubercle; but they have always been regarded as effects, and not causes, of pulmonary consumption; so that in Koch's finding a parasite (vegetable or animal) in tubercular matter there is no new thing, though it may have a new form, and has certainly been honoured by a new name. Further it is also well known to physiologists and others who have given special attention to the subject before us, that brain, pus, cheese, putrid muscle, etc., when inoculated will produce morbid results in various organs, *which cannot be distinguished from those produced by the inoculation of tuberculous matter*. How, then, can the morbid products of the inoculation of tubercle be considered as in any sense *specific*? In my humble judgment, all the results obtained by the inoculation of the so-called *bacillus*, as practised by Koch, were much more likely to be pyæmic in their nature, and such as the introduction of *septic* matters into animal bodies will generally produce, rather than anything unique or specific. But then it will be said, Mr. Editor, that neither you nor I possess the scientific faculty, and therefore, how should we know! The *Times* is unfair. It will afford ample space to any novel subject which happens to be popular, and

announce its birth too in a flaming leader, but its columns are generally too full for a rejoinder.—I am, &c., WM. DALE, M.D. Lond.”

A demonstration of Koch's tubercle preparations was recently given at King's College, on which the *Medical Press and Circular* comments as follows:—
 “The demonstration serves to convey to one a useful idea of the extreme minuteness of the data upon which Dr. Koch's conclusions rest.”

What, too, can be said of the exact and convincing results of experiments in which, again to quote Professor Tyndall, “three rabbits received each a speck of *bacillus*, culture derived originally from a human being afflicted with pneumonia,” and all three developed, not *pneumonia*, but *tubercle*! The inference may well be drawn and maintained, until further proof, in the face of the noisy outcry raised by the *Times* and the medical press generally, that “the true character of the most destructive malady by which humanity is now assailed” is yet to be determined, if the seed of pneumonia will produce a crop of tubercles. That is “de l'adaptation, Messieurs,” with a vengeance, and perhaps after all it will be eventually discovered or rediscovered that many different kinds of irritation will produce tubercle in well-selected subjects, such as herbivorous animals, who have tubercular disease with enormous frequency in their ordinary domesticated condition, even without inoculation of any kind.

At the very least, the jubilant attitude assumed by the pro-bacillar partizans of tuberculosis may be pronounced premature, so long as particles derived from a pneumonic lung, and particles of animal and vegetable dust, such as cantharides and lycopodium, all alike result in the production of tuberculosis.

CHAPTER XIV.

MUNK VERSUS GOLTZ AND OTHERS.

As a specimen of recent vivisectional literature in Germany, let us briefly examine Professor Hermann Munk's *Funktionen der Grosshirnrinde*, published at Berlin in 1881. The work is instructive; not only as dealing with the especial subject upon which, more perhaps than any other, the physiologists proper—as distinguished from the mere pathologist—bases his claim to be considered a benefactor of mankind; but as illustrating in a peculiarly lively manner the true state of physiological “science.” And from this point of view two points are more especially to be noticed: (1) the Professor's treatment of his fellow-workers: (2) his treatment of his victims; or as he calls them, his “Material for research.”

In a short introduction to the collection of seven lectures, of which the volume mainly consists, the Professor clears the ground for his own theories by disposing off-hand of the conclusions of such triflers as Flourens, Fritsch, Hitzig, Carville, Duret, Nothnagel, Schiff, Hermann, Ferrier and Goltz.

And here it is instructive to note the thoroughness with which the work is done. The half score of distinguished scientists with whom their equally distinguished *confrère* is here dealing represent as nearly as possible the ultimate outcome of physiological perfectibility so far as its potentialities have yet been realized.

And they all, as will be seen, contradict one another more or less flatly. The decisive authority with which yeas and nays, blacks and whites, inside-outs and topsyturvies, are here shown to be equally unskillful in manipulation, equally illogical in deduction and equally futile in result, is really humorous.

Here, to commence with, are a few specimens, culled *en passant*, with nothing very special about them, but just fairly characteristic of our professor's style of dealing with rival observers.

"For nearly half-a-century," says Professor Munk (p. 3), "Flourens' doctrine was accepted. One must now ask in wonder how this was possible!"

"Fritsch and Hitzig," he tells us, "concluded that Flourens' opinion was wrong." (P. 4.) "Hitzig however is quite certainly wrong." (P. 44.)

"Carville and Duret actually believed the disturbances, which appeared to them a want of spontaneity and directness in movement, to be a special kind of paralysis!" (P. 4.)

"Carville and Duret on the one hand, and Soltmann on the other, contradicted each other's assumptions." (P. 5.)

"There was nothing definite, *at that time*, to invalidate the conclusion of Nothnagel and Hermann." (P. 5.)

"Nothnagel said the disturbances proceeded from muscular sensibility, Schiff from sensibility of the skin; and on these grounds the motor function of the irritable portion of the cortex was altogether put out of the question." (P. 5.)

"Ferrier had no doubt about the correctness of his results; but his certainty is equalled by the impossibility of the slightest faith being placed in any of these

results by anyone who examines his researches. Only once did he arrive at the healing of the wound ; the animals for the most part only survived the operation a few hours ; at the utmost a few days. . . . The operations were rough, the observations rough, the conclusions rough. Very often the consequences of the operations were quite capriciously interpreted ; the distinctions between the effects of the extirpation itself, and the effects of the accompanying injuries—of the exhaustion, of the inflammation, &c.—were as capriciously drawn. The researches were nothing but confirmations, badly pieced together, of foregone conclusions. . . . In the doubtful warfare between friends and foes of localisation, these researches of Ferrier's, in due proportion to their worth, have attracted no further notice whatever." (P. 7.)

As Professor Ferrier is our own countryman, we may be pardoned for dwelling somewhat longer on his brother physiologist's opinion of him than we have thought it necessary to do in the case of other victims of Professor Munk's critical sword and spear.

"The simple perusal of Ferrier's researches," he continues quietly, "shows, that in every case the examination of the animals operated on was undertaken in a very imperfect manner. . . . The representations made by Ferrier are in no way to be distinguished from absolutely capricious interpretations ; and his further statements upon the temporary or permanent character of the disturbances induced by his operations, as also on the functional substitution of the one part of the cortex of one hemisphere, for the corresponding part of the other hemisphere are equally worthless."

And, finally, in a long note at the close of this lecture

he says, "A discussion which followed the reading of this paper in the Physiological Society led me to these remarks: for a question then asked showed me how Ferrier's statements entirely misled those who knew his researches only by reference. I hoped that those who were interested in the subject would be led by my criticism to look at the original. I found myself however mistaken. Most have not gone further than to Ferrier's "Functions of the Brain" as the source of their information; a book in which the author has in many places concealed the untenableness of his statements with great ingenuity. . . . Under these circumstances I need only refer to the extracts given below of the *best* of Ferrier's experiments, those on the Gyrus Angularis, as a proof of the justice of my criticism. Any addition on my part would be superfluous. And clearly as it is shown here, that all qualifications for the execution of such experiments were wanting in the author, my following lectures will further show that not even one of Ferrier's statements is founded on fact." (P. 16).

In his third lecture, the Professor again seizes and shakes his favourite victim.

"All these statements," he tells us, "and what depended on them as to the character of the disturbances induced by the operations, and recovery from them, were (as I said before) worthless, capricious interpretations of the phenomena; for the animals were examined by Professor Ferrier in a quite inadequate manner, and scarcely at all except at the time of general depression of the functions of the brain. If I had gone too far in making this declaration, when I had

only glanced through Ferrier's work, I should at once have repaired the wrong. But, instead of that, as the experiments have turned out, I said rather too little to you then: for Ferrier has not been lucky enough in his guesses to hit the mark even once, and all his statements have proved themselves false." (P. 37.)

And so having satisfactorily worried the life out of the last palpitating fragment of poor Professor Ferrier, our terrible Professor turns to Professor Goltz, whose friendly rivalry with that distinguished scientist—or perhaps we ought to say with his equally distinguished *alter ego* Professor Yeo,—formed so interesting a feature of the proceedings of the International Medical Congress of 1881.

And this Professor Munk, who has been only playing hitherto, looks upon as a serious task. Professor Goltz is really a person to be answered; and "though he is proposing to continue his own research, he finds himself obliged to deliver this lecture at once, in order to oppose the latest utterances of Goltz as soon as possible" (P. 10.)

"It is self-evident," he adds, "that Goltz's experience, when quoted against the localisation of functions in the cortex of the brain is of no worth." (P. 11.) "Professor Goltz's assumption, that irritation sets up inhibitory processes, having their seat in the cerebrum, which cause, through paralysis of certain centres situated in the cerebellum and its connections, all the non-permanent disturbances,—this assumption is inadmissible." (P. 13.)

Pages 79, 80 and 81 consist of comparisons between

the results of Nicati, Luciani, Tamburini, and Goltz. "Herr Goltz," says Professor Munk, "denounces the statements of the above-named Professors," and he proceeds to describe two experiments of Professor Goltz and his deductions from them with calm superiority. "But this," he concludes, "has nothing to do with the matter. I meanwhile proceeded in quite another way."

In a note to this his fifth lecture, p. 95, he says: "On the one hand, the admissions of Goltz in his lately published third paper (*Pflüger's Archiv*, Vol. 20) concerning localisation, and on the other hand the new discoveries which I was able to announce, showed clearly and forcibly enough on which side—Goltz's or mine—the questions were rightly handled, and views of the matter which accord with nature were won. Only personal interest however could have induced me to put my hand into the hateful brew, which Goltz had concocted out of uncritical mixing together of the results of Ferrier, Luciani, myself and others, in order to disport himself therein comfortably according to his manner."

Be it observed that the "hateful brew" here spoken of, and all the other harsh and contemptuous expressions above recorded have reference, not to "ill-informed fanatics," "hired scribes," "paid agitators," and miserable humanitarians in general, but to the Professor's fellow-workers, many of whom we doubt not are "venerable, some most illustrious, a few world-famous."

After these specimens of the treatment Professor Munk bestows upon his physiological brethren, it may

be supposed that his dealings with the helpless animals that form his "material for research" are sufficiently unsparing. And the anticipation without being disappointed. He maps out the living brain into "portions" A, B, &c., and proceeds to operate upon these various "portions" with the usual gentle applications of red-hot wires, electric "stimulation," and so forth. And then he recounts his pleasant experiences.

"The extirpation on both sides of the portion B, —the extirpation of only one side leads here to no certain observations—is such an exhausting operation, that animals thus mutilated have not yet been kept alive more than fifteen days." "Numerous experiments were made by secondary extirpation of the portions before and below A, on dogs which had been rendered soul-blind (*see below*) and had recovered." But all these experiments ended unluckily; the hemisphere of the brain, once mutilated, becomes so sensitive that every new attack upon it brings on violent meningitis* followed by death. Equally without result was another series of experiments. . . . The brain once mutilated is extraordinarily sensitive; violent terror, indigestion, traceable to meningitis proceeding from the injured part accompanied by superficial encephalitis" † . . . interfere in the most reprehensible manner with the prosecution of his enquiries. One dog, for example, had "not only lost his newly-won impressions of objects of sight for the second time, but showed himself to be perfectly blind, because it was very difficult to force him to move, he did not avoid the obstacles in his way, knocked against everything, and so on. In the worse cases, chronic

* That is to say inflammation of the covering membranes of the brain.

† That is to say inflammation of the substance of the brain.

convulsions and ataxy were added, in still worse cases, coma also. Only in the worst of all death supervened." (P. 24-25.)

By "soul-blindness," the Professor, splendidly superior to any slur on his philosophic Sadduceism, is careful to explain, he means in no sense what the word might be supposed by feeble superstition to imply. "When I repeatedly" he tells us, "defined soul-blindness=failing of visual representations of the memory-pictures of sight-perceptions, I regarded the use of 'soul' as exactly as unimportant as if I had said '*a* blindness,' or '*b* blindness.' The fact that casual readers have misunderstood me, makes me the less inclined to give up the word, because I have found no better one hitherto. Perhaps many of these people may become reconciled to the expression, because it is a very old one."

He continues, after describing the gradual progress of destruction of sight: "At last, when through encephalomeningitis the whole cortex of the hinder portion is destroyed more thoroughly than we can manage to do it with the knife, without the accompanying mutilation causing death, all perceptions of sight have at last ceased, and full blindness has supervened for ever." (P. 36.)

He tries his theories on monkeys.

"If the extirpation," he tells us, "be completed on both sides, the monkey is cortically blind. He sees nothing. By nature such a playful and active creature, the monkey sits from thence onwards quite apathetic, and as if dreaming in his cage, without stirring, for

hours, till some noise rouses him into terror. If taken out of his cage, he does not move from the spot; and, if forced to move by blows, he strikes against obstacles, falls from the table, and so on. . . . It was very interesting that, after two months, a hemiopia was distinguishable." (P. 39.)

"Once or twice, when I had extirpated the entire cortex on the upper surface of both temporal lobes, I observed persistent cortical deafness; but since the animals" (these were dogs) "only survived the operation eight days, and were in a bad condition, besides, one cannot lay stress on these examples." (P. 41.)

"Let us examine," he proceeds a little farther on "a dog from whose cortex a large part inside the portion D, let us say of the left hemisphere, has been extirpated. . . . When the fever is past, 3 or 5 days after the operation, we observe as follows:—Loss of sensibility in the right fore-leg. If we touch any of the other three legs, or prick them slightly, the dog looks round, or bites if he is bad-tempered; . . . if we do so to the right fore-leg, even if we pinch or prick severely, the dog takes no notice; and if we pinch forcibly, or prick deeply, the dog only lifts his leg, and neither looks round nor tries to bite." (P. 45.)

He mentions casually, "the alternate sewing up of the eyes" of his monkeys. A note at p. 54 explains—"The wildness of monkeys does not allow of a simpler mode of occluding one eye. We have only in the last few years succeeded in taming one or two so far as to allow of a good adhesive plaster. I notice in passing, that of the different sorts of monkeys

I have used, *Macacus Cynomolgus* is the best for research ; *Cynocephalus* is too large, *Inuus Rhesus* too wild, and *Arcocebus Sinicus* has too little power of resistance." (*Zu wenig resistent.*) This means of course that the poor little creature succumbed inconveniently soon under its suffering. But we translate straightforwardly.

Professor Munk opens his fourth lecture by saying that "after a great number of fruitless experiments," he did "twice succeed with dogs, and twice with monkeys bilaterally, in such thorough-going extirpation of the visual centre, that the perceptions of objects were not re-formed, and the power of receiving visual impressions was damaged permanently. The rarity of the result was only too easily to be understood, since such large continuous extirpations of the cortex offer considerable experimental difficulties, and are, besides, about the utmost the knife can venture on doing to the cortex, if the subjects of the experiments are to be kept alive." (P. 57.)

"A year and more," says the Professor, triumphantly, "has this Stirnlappen defied all attempts to get a glance into its functions ; for, after its removal, no disturbance whatever was observable in the animals operated on ; but at last it has surrendered !" (P. 61.)

"Up to the present time," he continues, "I have only been able to procure 29 monkeys for purposes of research. Of these I lost 8, partly through illness, partly through the consequences of the first operation without result. With the remaining 21 I got 50 experiments, for most of the animals survived two

or three different operations, one or two went through four, and at last one endured five, generally separated from each other by intervals of some months. Of these 50 experiments perhaps one-third on the visual sphere and two-thirds on the sensory spheres failed. If this number of experiments seems small besides the hundreds I have made on dogs, yet the smaller number is of greater importance, because I had got through the inevitable tentative attempts upon the dogs, and wasted none of the monkeys." (P. 63.)

Happy monkeys! Considerate and economical Professor! Nothing "wasted" but a few cheap dogs. How great the privilege of standing high in Creation's scale—and costing pounds instead of shillings!

And so we proceed with our carefully economised "material." We are dealing with a monkey whose left brain has been subjected to the Professor's judicious manipulations.

"As soon," he informs us, "as one touches the left eye with a needle, winking and violent play of the facial muscles succeeds; the monkey, with expressions of terror or anger tries to draw back its head or turn it aside, and almost invariably strikes out with its left arm at the assaulting hand." (Poor little thing! No wonder.) "But if one does the same to the right eye, there is nothing but winking, and one can press and prick as long as one likes, the creature keeps still. . . . When one has bound up the left eye of a dog, or sewn it up in a monkey, the creature makes bad shots at the pieces of food laid before it, the more noticeably so, the smaller they are." (P. 65.)

We should scarcely have expected to find the well-

known school-boy trick of "cover your eye and hit my finger" elevated into a physiological experiment. But then, to be sure, we did not sew up the eyes of our school-fellows to arrive at the result. Nor had we previously taken the precaution of drilling out a portion of their skull and "permanently damaging their visual centres" by way of scientific "hedge" against any failure in the experiment. But the result was much the same. One would have thought the Professor and his assistant Bartel, "to whose careful tending," he touchingly adds, "I owe it that my dogs have been kept alive, in spite of all dangers, most of them for two or three months, some even for four months, after the second operation," might advantageously have played the game together in the laboratory, while waiting for their victims to regain strength for their further torments. It is generally admitted that human "material" is preferable for research—when it can be got.

Herr Munk is much hindered by the natural sensitiveness of his unhappy monkeys. And is greatly vexed by their inconsiderate behaviour.

"Although," he says, "we have busied ourselves for months at a time with most of these monkeys, we have never been able to manage any one of them, nor even partially to tame them; the unconquerable terror of the animals invalidates all investigation as to their power of sensation; for they either behave quite wildly, or, when forcibly held down, let anything be inflicted on their skin without resistance and quite apathetically. The consequent imperfection of the results is certainly to be regretted, and would have been of consequence if the monkey had been our first and only animal experimented on; but it does not signify, since we have been able to

follow all the phenomena of sensation with precision on dogs." (P. 67.)

Does not signify! And was the terror and suffering of these miserable monkeys,—their wild fear and their utter prostration when conquered and "forcibly held down," and in the power of the undisturbed Professor inflicting "anything" to rouse them to manifestation of pain—was this really and truly of no consequence whatever, to him—or us—or—?

Let us proceed with our Professor's story, as it leads us on through fresh experiences. The total extirpation of one entire sphere of sight "is difficult," he says. "The animals very frequently die in the early stages from hæmorrhage, inflammation, abscesses, discharge into the ventricle, etc. Still, by patience and practice, I have made myself master of the operation, and after cutting away much more of the cortex than usual, I have kept seven dogs well over the healing stage for thirteen weeks." (P. 81.)

And when "by patience and practice" he has kept his mutilated victim alive through those three months of agony, this is the notable result.

"If one burns his nose with a lighted match or strikes him with a stick, later on he will throw back his head if one presents either of them to his left eye." (P. 83.)

In the sixth lecture we are again told of the difficulties presented to the inquirer by "subsequent hæmorrhage and inflammation. Some of the animals die in the first few days from these. The second week another set die from sudden convulsions and coma, and

if they do not die of encephalomeningitis the object is yet lost, because the lesion of the cortex has become too large. . . . In the last series of 30 dogs, 19 made my researches useless by hæmorrhage or inflammation, mostly after the first, fewer after the second, operation. Still the successful experiments pay richly for all the trouble. For, from the moment the second visual sphere is removed, the dog is and remains perfectly blind; he has lost the sense of sight at once and for ever." (P. 98.)

Wonderful discovery! Rich payment indeed! When you have successively severed the G string, the D string, the A string, and the E string, your violin will make you no more excellent music. Only those can be replaced. The delicate mechanism through which the living creature receives the harmonies of light and colour cannot be renewed. For him is true what the Professor's noble countryman, Schiller, says, "He must sit, feeling, in the night. . . . To die is nothing, but to live and not to see, that is an affliction!"

Yes; "if you throw the keenest light into his eyes and suddenly overflow this or that portion of his retina with brightness, he is unmoved." . . . "Only when forced will he go up or down stairs, feeling each step with his muzzle; if he cannot feel it he will submit to any ill-treatment rather than move a foot. You cannot force him to run or to spring. If you put him on the table so that his feet are close to the edge, he will fall off regularly if he is made to move." (P. 99.)

No doubt Professor Munk has done his work completely. We may be very sure no foolish sentimental scruples have interfered with the perfection of his results.

And so we come to a fresh claim of our Professor, on the honours of Scientism and the gratitude of mankind. "To keep alive these valuable animals," he pathetically assures us, "for a long series of observations is another difficulty of our research. The mutilated portions of the cortex are, as I have said, extremely sensitive. Terror and fear, such as the trials of their powers bring with them; attacks of the lungs or bowels, which would be of no importance with other uninjured dogs, even simple indigestions may prove fatal." . . . (P. 100.)

Alas! Alas! Who but must sympathize! With the dogs and monkeys in their perversely premature death of agony and terror? Surely not. With the Professor, of course, whose powers of infliction are so cruelly limited.

At page 111 there is a detailed account, too long and too heartlessly indifferent to quote, of the "trials" of powers spoken of above. Burning, striking, pricking, whipping, &c., are gone through in succession from the first week to the fifth to try the perception and the memory of the mutilated victim. And the feeble creatures *will* succumb too soon. "*Zu wenig resistent*," every one. One quite feels for Professor Munk. He is before his time; out of his place—in this world.

The concluding lecture is mostly taken up with polemics against Professor Goltz's "reprehensible" views of the cortex of the brain (p. 128) and against Professor Ferrier's "fancy pictures" of brain centres (p. 129). Also with a terrible experience with one unfortunate dog, whose torment began on the 1st of November. A second operation followed on 22nd December; a

third on 27th December ; it sickened on 2nd January, recovered on 6th January, and was poisoned with prussic acid on 24th March. Its brain was in one part " all very beautiful and healed according to the usual rules." Into the rest of the description we will not enter.

We close our review of this book with one more instance, not because it is more cruel than the rest, for it is not, but because it made an impression on ourselves as we first read through the book, which was not effaced or even weakened by anything we afterwards saw.

It will be found in a note on p. 16.

" 18th November. The left Gyrus Angularis burnt and destroyed by galvano - cautery. The left eye securely closed with plaster. The monkey allowed to recover from the chloroform.

" After a few moments it begins to make efforts to rise ; cannot however stand. Half an hour later it sits up, and begins to feel around its place, cautiously ; but does not attempt to move. Does nothing when a light is brought near its eyes. Does not resist when lifted up and its face brought close to the light. Its sense of hearing and of feeling are preserved, for it starts up at a noise and shrinks when pinched. Put into a cage with two others, it hangs itself to the bars and takes no notice of its companions. It will not move from its assumed position.

" A little later it seats itself in the cage and covers its head with its hands."

The picture would be weakened by many words.

Poor unresisting helpless thing! One feels the "tender, bewildered sympathy" of Cardinal Newman as one realizes its mute appeal; covering its face as it sits.

And it was not Professor Munk who mutilated that helpless creature and recorded its pathetic action. The cage where it sat down and hid its face was not in a German laboratory. The thing was done in England; and the man from whom the little creature shrank and covered its eyes was Professor Ferrier.

CHAPTER XV.

GOLTZ VERSUS MUNK AND OTHERS.

Ueber die Verrichtungen des Grosshirns. By PROFESSOR GOLTZ, of Strasburg. 1881.—This book consists of four treatises, published originally in Pflüger's Archiv., in May, 1876; December, 1876; June, 1879; and September, 1881, respectively. The first two have long been known, at least by extracts, to our readers. It will be sufficient to remind them of the "delicately-formed little dog," on whose paws the Professor successively screwed iron clamps,—allowing it to rest once during the process "for fear of dangerous reflex actions,"—till "the creature howled piteously, and after a short time foamed at the mouth." (P. 47.) They will remember that "under pressure, the brain shoots out of the hole like a mushroom" (p. 5), and that after the Professor's humane manipulations "the appearance of the brain is like a lately-hoed potato-field." (P. 49.)

Into the experimental part of these first two treatises we will not, therefore, enter at length. Postponing the account of his dealings with the animals which were the victims of his research, we will examine for the present only the Professor's treatment of his physiological brethren.

To make the following extracts easier of comprehension, we will state as shortly as possible the three

theories which Professor Goltz desires to overthrow ; and will then quote his objections to them in succession.

I.—*The Theory of Localisation.*

The functions of the brain—the upholders of this theory tell us—seeing, hearing, tasting, smelling, feeling, etc., have each a special centre. Munk, the latest speaker on this side, divides the brain into A, sight ; B, hearing ; C, D, and E, sensation in hind feet, fore feet, and head respectively, etc. These are again sub-divided into A, A₁, A ; B, B₁, B ; etc. The central portion being that in which is specially located the power of receiving memory-pictures, so that the extirpation of A₁ produces what Munk calls soul-blindness, or the incapacity for realising the meaning of impressions mechanically conveyed to the retina, while the extirpation of A produces “cortical blindness.”

Fritsch and Hitzig started the idea of localisation ; which has been adopted, with various modifications and extensions, by most experimenters, as we shall see. The opponents of the theory ask how it is that when any of these centres are extirpated, and the victim is kept alive long enough, the functions apparently return ; as it seems after long contradiction, and alternate assertion and denial, to be pretty well established that they do.

To meet this difficulty is proposed :—

II.—*The Theory of Restitution.*

By this theory it is suggested that perhaps the centres grow again.

Hitzig and his followers think so.

III.—*The Theory of Substitution.*

By this it is suggested, on the other hand, that the functions of the extirpated centres are carried on by the other, uninjured, parts of the brain, which act for the parts destroyed.

Carville, Duret and Vulpian think so.

These three theories—the last two being explanatory of and dependent on the first—Professor Goltz is engaged in demolishing. And it must be owned that he makes thorough work of it.

"All my predecessors," says the Professor, "Hitzig, Ferrier, Carville, Duret, Soltmann, and others . . . confirm the old statement of Flourens, that the loss of a considerable portion of brain-substance produces no injury in the functions of this organ.

"But from a great number of researches which I have spoken of above, I decided that Flourens was unquestionably in the wrong." (P. 82.)

"Flourens notoriously affirmed, that creatures whose brain had been destroyed became entirely blind. Longet denied it ; but thought that such creatures could not use their sight-impressions for practical purposes.

"I have proved that this view is untenable."

"Schiff, to suit an old theory of his own, speaks always of mere disturbances of taste, and denies the existence of disturbances of sensibility.

"This assumption is, however, indubitably untenable."

"Almost all writers deny that mere mutilation of the brain can affect the sight. Schiff says distinctly that even the removal of one whole half of the brain has no effect on the eye. Hitzig alone notes shortly that after

mutilation of the posterior lobe, the opposite eye becomes blind.

"We shall see that this statement contains only a little portion of truth." (Pp. 14-15-16.)

"At the beginning of these studies," continues Professor Goltz, "I proposed to myself to test the statement of Flourens, who affirmed that great loss of substance, on both sides of the brain, had been sustained without permanent disturbance, since the uninjured portion takes upon itself the functions of the part destroyed.

"I look upon my task as in so far completed, that I have contradicted Flourens' statement. . . . I think I have also proved by my researches that another assumption of Flourens' is erroneous." (P. 71-72.)

Professor Goltz evidently does not think much of Professor Munk's ingenious "B, B₁, B," or area of "hearing" faculty.

"The fact," says he, "that in my researches I have always observed disturbances of sight, and no disturbances worth mentioning of hearing, may be accounted for by supposing that the portion of grey cortex, which serves"—according to the localisation theory—"for hearing, is beyond my reach. The centre of hearing might possibly lie at the basis of the skull. But it seems to me unprofitable to follow such possibilities even in thought. I shall wait till some one shows me the portion of grey cortex (graue Rinde) where the sense of hearing is enthroned." (P. 73.)

"I am far," proceeds the Professor, "from sharing the view of Ferrier and Munk. Their distinction between movements which occur purely as reflex

actions in consequence of sound-impressions, and actions which are induced by memory of sound-impressions, seems to me altogether capricious." (P. 91.) "Hitzig, Ferrier and Munk all three accuse me of employing a method which is quite incapable of bringing out any conclusion about functions of limited portions of the cortex. Hitzig calls my method of research a roundabout way. But," pursues Professor Goltz, "how little these specialists have done is clear, from comparison of their results. Hitzig places the muscular consciousness; Ferrier, the centres of voluntary motion; and Munk, the sphere of sensation, *in the same lobe*. I will only remark that I had a full right, on the strength of my former researches, to express doubt concerning the admissibility of the doctrines of Hitzig, Ferrier, &c. Considering the great noise which the idea of localisation has made, it does not seem to me superfluous to point out the great weakness, contradiction and inconsistency contained in this teaching." (P. 102.)

As an instance of the said contradiction and inconsistency, Professor Goltz allows us a glimpse into the opinions of the initiated circle and their respective divergencies.

"Ferrier," he tells us, in the course of his third lecture, "is well known as giving a greater extension to the so-called excitable zone than Hitzig. Hitzig accuses Ferrier of employing too strong currents. But we have no criterion as to how strong exciting currents ought to be. We cannot hit upon a distinction between proper and improper centres, or points of excitation. Ferrier believes that the movements he obtains by the excitation of certain points are of a reflex kind, and

mentions other points, the excitation of which stimulates directly motor centres. Luciani and Tamburini justly object that the distinction is purely arbitrary. It is free to all to hold the view of Schiff, that all such motions, obtained by excitation of the cortex are reflex."

Professor Goltz then goes on to compare the opposing theories of Hitzig and Schiff, and concludes, "So that after all Hitzig's view will fall in with the view of Schiff, which he has so strenuously combated." (P. 103-104.)

Who shall decide when doctors disagree? The prospect is not encouraging. But Professor Goltz seems to be quite aware of the state of opinion, which reminds us of the stormy ocean in Shelley's "Fugitives"—

" Withdrawn and uplifted,
Sunk, shattered and shifted
To and fro !"

For, he tells us, "it is not often that in matters of physiology of the brain, two men are of one mind." (P. 9.) This confession reminds us of a speech of Mr. Bright's, on the Ecclesiastical Titles Bill: "There is another remarkable point in this matter," he said. "It has been observed, '*Multæ terricolis linguæ, cœlestibus una.*' But it does not appear that the celestials in this House are more agreed about the matter than those who feel little regard for either side of the question."* The "Celestials" of modern science certainly do not seem agreed upon their language. Even Professor Max Müller would find it hard to reconcile their tongues; it would require Cardinal Mezzofanti to understand them.

* Bright's Speeches, vol. 2, page 482.

"On one point, however," says Professor Goltz, "almost all writers are agreed; that the sensitiveness of the skin is not affected by mutilation of the brain."

We sigh with relief,—one point at last,—"land, though but a rock, draws nigh!" Oh! dear, No.

"This proposition," continues our Professor, quietly, "is, nevertheless, erroneous. Hermann only, and in one case, has given the fact correctly." (P. 9.)

Now we will pass to Professor Goltz's contradiction of the second, or restitution-theory.

"Hitzig and his followers have asserted"—shortly, that when after removal of supposed centres the functions returned, the centres must have grown again.—"We are now in a condition to show the untenableness of these hypotheses." (P. 36.)

He states the restitution-theory and remarks, "Here some of my readers will smile and wonder that I think it fitting to express such nonsense. I am quite ready to join in their laughter; but my statement is not uncalled for, nevertheless, for certain modern champions of the physiology of central organs allow centres to take root and grow as freely as mushrooms. I myself certainly regard the notion that a nerve-centre can be entirely newly formed much as if anyone were to tell me that when a man's leg is amputated a new leg will grow." (P. 79.)

"Equally untenable seem to me the views of Hitzig. Nothnagel has already pointed out the *non sequitur* in Hitzig's argument." (P. 38.)

"The same objections which I make to Hitzig's teaching apply obviously to Ferrier, into whose researches I think it superfluous to enter." (P. 39.)

Poor Professor Ferrier! Munk calls him names, as we saw just now, and Goltz will not even condescend to read him!

And so our Professor passes to the third, or substitution-theory:—

"Carville and Duret think with Vulpian,"—shortly, that the new centres which carry on the restored functions have their seat in the portion of the brain which has not been injured, which supplies the want.—"To pursue this hypothesis further is superfluous." (P. 37.)

"Soltmann has shown that experiences on men contradict the theories of Carville and Duret. . . . As Carville and Duret are contradicted by him, they in their turn have victoriously set aside the hypothesis of Soltmann." (P. 38.)

"If Carville and Duret say, that the uninjured part of the brain is substituted for the mutilated portion, then, as Richet, Luciani and Tamburini clearly show, the principle of localisation is given up. . . . Then Flourens' principle is substantially acknowledged. . . . But the assumption of Carville and Duret is, as I showed in the first treatise, insufficient. . . . Still easier is it to show the intrinsic untenableness of the hypothesis newly set up by Munk. . . . The one fact alone, that a dog, whose left centre has been squirted out, can recover the power of giving his right paw, contradicts

Munk's theory. . . . Ferrier's doctrine, that only the so-called symmetrical centres can carry on the substitution, is contradicted by facts. . . . The hypothesis of localisation cannot expose itself more utterly than by needing such a defence. . . . Luciani and Tamburini did not escape this necessary consequence." (Pp. 107-8-9.)

The Professor is now well within his adversaries' defences. And pushes on, jubilant.

"Now," says he, "we will follow up the war of annihilation against *all* that localisation doctrine which constructs a system of small circumscribed centres for the various functions of the brain. . . . How can I energetically enough protest against the idea, that the expelled centres take refuge in the deep ganglions of the brain! . . . I at least do not believe in miracles. . . . I hope to live to see the day when all these delicately wrought modern hypotheses will be buried in the grave where Gall's phrenology rests so quietly." (P. 110.)

We might say with Paracelsus that the Professor's—

"Intimations rather fail

In clearness than in energy."

"It is impossible," asserts Professor Goltz, "to produce permanent disabling of any muscle, by any mutilation which is confined to the cortex (Rindenschicht) of the brain. All teaching which contradicts this axiom, for instance, the assertions of Charcot, rest upon insufficient or erroneous observations." (P. 113.)

"My view stands naturally in sharp opposition to that of Hitzig and his followers," pursues the Professor.

He then quotes Hitzig's statement of his theory of circumscribed centres, concluding with the trenchant remark, "Hitzig has again and again repeated this statement as the quintessence of his teaching. The statement, as I have shown, is contradicted by facts. . . . Luciani and Tamburini admit that they cannot distinguish between motor and sensory centres, at certain points. The localisation-principle of circumscribed centres is therefore sacrificed by the authors themselves." (Pp. 115-116.)

And finally after showing how Munk, Ferrier, Luciani, Tamburini, and Lautenbach contradict each other, Professor Goltz sums up in the astonishing remark, "These gentlemen, despite their contradictions, are all right and all wrong." (P. 116.)

We confess that at this point "our wits begin to turn," and we wonder if anything has happened to our own brain-centres. We suppose not, however, for we seem to understand the further statement of Professor Goltz:—"I proved in a former treatise that the hypotheses, brought forward by the disciples of localisation to account for the recovery of lost brain-functions, must be thrown aside, because of their contradiction to sense. Now comes the question whether we can give a solution of the facts." (P. 119.)

Professor Goltz then develops his own theory, going out of his way to deal blows right and left at Hitzig and Ferrier, like Robert Bruce among the men of Lorn, and concludes thus:—

"'But this would bring us back to Flourens' starting-point!' cries Hitzig. I gladly subscribe to this state-

ment, and am not alone in so doing. Brown-Séquard and Dupuy combat the new localization-doctrine with the same weapons that I do; not, however, that I agree with all the details of these authors." (P. 124.)

Oh, no; so we should suppose. The aloë flowers but once in a century. Two suns in one sky, perhaps. But not two physiologists in one mind. And one of those Professor Goltz. This autocrat of all the laboratories is not serene and sublime on his lonely pedestal, nevertheless.

"Hitzig says in his latest utterance, 'What is to be held concerning the completeness of restitution, is taught by the above-described double experiment.' I should have expected," remonstrates Professor Goltz, "that the sentence would have run, 'is taught by the researches of Goltz;'" for it seems to me clear, that Hitzig has only confirmed by one experiment, what I have proved in two great works. What Hitzig thought about restitution *before* my researches were published is plain by what he said at that time . . . but I am sufficiently avenged, as he has now changed his mind. *Perhaps he has fallen into the opposite error in the recoil when he asserts*" . . . etc. (P. 125.)

"Munk," he goes on to tell us, a little farther on, "has described the disturbances of sight and sensation in dogs with mutilated brains exactly as I did. If he does not mention my name, he probably assumed that my works are well known to his readers. . . . But Munk's deductions contradict altogether the most common facts of experience." (P. 126.) "While Munk gets rid so easily of the question regarding the seat

of intelligence, Richet believes experiments on animals to be useless for the point. I hope this author, when he has perused my work, will own that experiments on animals have brought us very near."

"Ferrier," he proceeds, "reproaches me with not having read him in the original. That was true at first. Meanwhile his larger work has appeared, and I have read it carefully, both in the original and in translation. The book is of worth because of the size of its contents; but I must repeat that Ferrier's researches on the brain are throughout inconclusive." (P. 127.) "Hitzig affirms that certain centres regulate muscular consciousness: Ferrier says they are psychomotor centres, Munk places the sphere of sensation in them. All these hypotheses"—it is almost superfluous to add—are, the Professor tells us, "contradicted by facts." (P. 163.)

In the last few pages of his fourth treatise the Professor gathers himself together for a duel à outrance with his especial enemy Professor Munk.

The "hired scribes" and "paid agitators" stand by in awe-struck wonder, and endeavour to learn, as lookers-on may, a few cunning tricks of fence from these "venerable, most illustrious, world-famed" champions.

Like Tasso's Rinaldo, "with a hand still masterful though in fury," Professor Goltz attacks his foe. "In his lately-published book"—reviewed in Chapter XIV.—"Munk throws the accusation at me that I have concocted a 'hateful brew' out of the researches of himself, Ferrier, Luciani, &c. Since all proof is wanting, I do not know which of my remarks is supposed to be

incorrect. The tone of the attack might render an answer unnecessary. But since I hear that the romantic doctrines of Munk enjoy some consideration, are spread abroad by means of popular lectures and text-books, and produce confusion in people's heads, I think I had better speak about some points of Munk's book. . . . Anyone who has read my first three treatises and those of Munk in chronological order must be astonished at the quantity of familiar matter which he finds, again without acknowledgment, in Munk. . . . Munk has with the greatest caution used my observations without mentioning my name. . . . I must accuse Munk of having spoken a sentence before a meeting of the Academy, in which he has taken credit for what is not his own. I ask an impartial reader to glance at Treatise 1, pp. 15-27, and to convince himself that I have so described what Munk calls 'soul-blindness,' that it only remained for him to *Christen my child!*" (The italics are the Professor's.) . . . "Perhaps the reader will say Munk's doctrine hides under all these coarse crudities some little grain of truth. I have not found it at present. The pathologists of animals have not been able to verify Munk's theories." (Pp. 175-176-177.)

"An animal with extensive destruction of the upper cortex becomes distinctly weak in smelling: therefore the gyrus hippocampi cannot be, as Munk says, the sphere of smell.

"The entire sphere of sight (so called by Munk) can be removed without the animal's becoming blind. This imagined 'cortical-blindness' is therefore a mirage.

"The entire sphere of sensation can be destroyed without the animal's losing sensation in any part of its

body. Munk's 'cortical insensibility' is therefore a nonentity."

And thus we leave Professor Goltz, master of the field, standing among his prostrate foes, and chanting his victory-song ". . . . Heaps upon heaps, have I slain a thousand men." Out-Samsoning indeed the mighty man of old in that he has not even had to borrow the weapon of his vengeance.

CHAPTER XVI.

GOLTZ VERSUS MUNK AND OTHERS.— CONTINUED.

Having thus far dealt with Professor Goltz in his manipulation of his rival observers we will now lay before our readers, a few, necessarily very few, instances in detail regarding his treatment of the animals on which he made his researches. In a book consisting of 177 closely printed pages, any selection can obviously give but a very imperfect impression of the contents.

We must pass rapidly over the first and second lectures; they were delivered in 1876, and extracts from them have long been familiar to those of our readers who are interested in the question of scientific torture. At the very outset Professor Goltz claims for himself a ghastly distinction:—

“No one has hitherto succeeded, as far as I know the literature of the subject, in obtaining such thorough-going destruction of the brain while keeping the animal alive, as I have. I have succeeded, by a series of syringing operations, divided by certain intervals of time, in so mutilating one half of the brain, that the convolutions of the whole surface of the shrunk-up organ next the roof of the skull had disappeared. The animal has lived for weeks with the brain thus mutilated, and has served for numerous observations.” (P. 8.)

And the result of these observations our Professor accordingly proceeds to lay before us. The "operation" itself is by this time sufficiently familiar, consisting invariably of the destruction of this or that area of the brain, laid bare for the purpose by the cutting, or breaking away, as the case may be, of a sufficient portion of the skull.

"On the day after the operation," he observes, "one can generally obtain movements, if one sharply pinches the paws which seemed without sensation the previous day. By stronger pressure, by sticking needles in, and other like devices, one can easily extract signs of pain from any part of the skin. After this I tried with weights, whether on the right side less pressure was required than on the left to make the animal scream." (P. 11.)

Professor Goltz found that after injuring one side of the brain, the opposite eye became affected. He wanted to observe this injured eye, and finding it difficult to prevent the use of the sound eye, because the animals pulled the bandages off,* "I determined," he says, "to destroy the sound left eye in several dogs, in order to estimate rightly the functions of the injured one."

So he began with the initial "operation" on November 8th.

On the 11th of December he destroyed the sound eye.

On the 10th of January he subjected the dog to a third operation.

* Professor Munk, it will be remembered, gets over the difficulty by sewing up the uninjured eye.

On the 5th of February he operated again.
On the 15th of February the dog died of meningitis.

Another dog was subjected to the simultaneous squirting out of part of the left brain and extraction of the left eye, on the 29th of November.

On the 12th of January a third hole was bored in his skull, and more of the left brain squirted out. The result of this was that "he was not afraid of the sight of the whip from which he had before run away. But when the whip was cracked, he crouched terrified in a corner."

On the 29th of January "he was subjected to a third operation. Two new holes were bored, and a great quantity of brain-substance squirted out."

On the 2nd of February "his pupil contracts with great energy, when his head is held to the sun. The dog makes no resistance, though the rays must certainly irritate his membranes painfully."

On the 10th of February "the fourth operation is undertaken. Since there is no room to bore more holes, a large part of the skull is broken away, between the old and new holes. The rest of the grey substance is then squirted out."

On the 21st of February "a new and important series of experiments is begun with him." We cannot go into the details. Holding him out of a window many feet from the ground to frighten him, forcing him by blows and by playing water-engines on him to run against obstacles placed in his way—all manner of ingeniously-contrived devices are employed to worry and torment the poor mutilated victim. We must refuse to follow the Professor farther through his cowardly diversions.

On the 4th of March the dog is again subjected to operation. We breathe more freely when we learn that "on the 8th of March the dog died of meningitis." (Pp. 20-24.)

On pp. 31-33, we find another "series of experiences" varied with operations, which latter took place on December 1st, January 13th, February 15th, March 6th; the victim died on March 7th.

"In this particular set of researches," says Professor Goltz (p. 45), "I have not yet been able to perform more than four operations upon the same animal, because the animals always died during the intervals."

We all remember the grisly story of the House of Atreus, Professor Goltz appears to have studied it to highly practical purpose. There is a potentiality of cannibalism in man which in the canine nature is less readily realisable. "Dog will not eat dog" even for hunger's sake. To debase him to that level is a triumph reserved for the scientist; and even for him it is not always attainable.

"Unmutilated dogs," the Professor tells us, "refuse to eat dog's flesh; they would rather starve than touch it. . . . There are however some stupid young mongrels who do not object to eating their own race. So that if you wish to try whether a dog with mutilated brain no longer shows this loathing, you must have made sure that he showed it before. Two of my subjects who did turn with disgust from dog's flesh before, have ceased to object since the operations. But other mutilated dogs, even those that had endured an extraordinary loss of brain, have entirely refused, and could by no means be forced to still their hunger with the flesh of their own kind." (P. 54.)

Apparently this modern Atreus has found the instinct stronger in his ill-used victims than the father of the Atridæ found it in his unhappy brother Thyestes, —and he had no circular saw to help him.

Another most interesting result from the destruction of the organ by which the intelligence is governed is found in the amusing incapacity of the mutilated animal for taking his own part in the "struggle for existence."

The Professor keeps all his dogs together, and tells us "Creatures thus deprived of their powers"—by his ingenious manipulations—"are helpless in the struggle for existence. All the food is carried off by the uninjured dogs. If the dogs who are confined together take to fighting, the mutilated ones get all the bites of all the others, but cannot avenge themselves, because in spite of their rage they cannot hit upon their adversaries." (P. 67.)

"Still more instructive is the following experience," say Professor Goltz (p. 74): "The delicately-formed little dog, on the left hind paw of which I had fastened an iron clamp and left it there, on November 16th,* and which suffered afterwards from reflex foaming at the mouth—this dog sickened immediately afterwards with appearances, as if one had performed a fresh squirting out of brain on the left side. The animal, which before the experiment with the clamp could move and run quite normally, was again lamed on the right side, so that it could neither move nor even stand. This effect only lasted 24 hours; the weakening of the muscles on the right side quickly disappeared, and the

* See ZOOPHILIST for June, 1882, where the former experiment is given with reference.

animal could walk as before. The dog is still alive on the 5th of December."

"I have for a long time possessed a dog, which, having endured very extensive mutilation of both halves of the brain, is not very distinguishable in his behaviour from a frog which has suffered a like mutilation. One may describe this dog as a complicated eating reflex-machine." (P. 82.)

The Professor is very proud of one particular dog, which "during the course of the year 1877 was subjected to four great operations;" and he describes the symptoms at great length, which the dog now shows, in May, 1879. We cannot go into them all; but we note that "The creature cannot be brought to show sympathy, joy, surprise, displeasure, or fear." (P. 89.) The poor thing "will eat dog's flesh; if one even gives him the entire carcase of one of his former companions, he will tear it to pieces and feed till he is satisfied. A healthy dog," adds the Professor, repeating his former remark, "would rather starve for a succession of days than touch the body of a comrade." (P. 92.) A noble triumph indeed for the Professor's science. A triumph hitherto unparalleled in this undeveloped world. Count Ugolino eternally gnaws the skull of the man who starved him and his four kinsmen to death in the Hunger-Tower;—but it is in the Inferno.

Nor is this the only victory he wins over the weak beneficence of Nature. He conquers not only the chivalrous comradeship of this "lower" animal, but his capacity for personal enjoyment.

"There is no means," he tells us with a proud sense of the achievement of an Event, "of bringing the dog to a manifestation of pleasure. He never wags his tail, or shows that he cares for food. If one strokes him, he remains equally indifferent."

"One might think," pursues Professor Goltz, "that the creature is of a harmless nature, and incapable of any passion. But this would be quite a mistake. If one tries to lift him up, he becomes fearfully excited, grinds his teeth, bites and struggles with such force that one must be a powerful man to hold him. One may, by diverting his attention, lift up his two front feet and, cautiously, one hind paw. But the instant one lifts the fourth paw from the ground, the outbreak of fury begins. As quickly as the attack of rage takes place, so quickly is it over; as soon as one sets him on the ground, he is quiet, and goes on feeding as if nothing had happened. He becomes wild also if one takes his fore-paw in one's hand and holds it fast. First he struggles to release it; then he gets angry and bites, but cannot find one's hand and bites his own paw. He is very angry if one lifts him by the scruff of his neck, or pinches him. He springs furiously aside and bites his own fore-paw." (P. 96.)

"I am dead to peace and joy:" he might say like the Little Master in Sintram, "I live for pain and anguish."

And Man, his master, who stands to him in the relation in which God stands to the human race, is every day playing towards him this cruel part. In God's Name, by what right?

When man imprisons, enslaves, tortures his fellow-man, evil and depraved though the persecutor be, the victim has consolations—courage, virtue, the sense of

injustice nobly borne, the hope of compassion and applause when the time comes that his cause is known; and yet more the certainty, assured to him by the best and noblest of his race, that this life is not all, and that there is light beyond the grave. Though he be not a willing victim or a self-devoted martyr, all these are at least alleviations of his pain.

But, as far as we know,—and modern physiologists at any rate will not dispute the matter with us,—the animals have their be-all and their end-all here. For them, not the “*summum bonum*” alone, but *all* good is in the pleasures of the present; their only joy is in the enjoyment of mere life. To take this from them, to make their innocent lives a sacrifice, prolonged as we have seen for years in deadness to all but pain, surely this is a wicked use of the power given us over them. Is knowledge bought at this price, science served by these means, ennobling, beneficent, God-like? And once again, what right has Professor Goltz to do this thing?

That he *can* do it unchecked, and *does* do it unchecked, the next extract will show.

“To construct a machine,” he says, “by which needle-pricks can be inflicted with great force and in rapid succession, is not difficult. One can easily employ the principle of the sewing-machine. But even so, the loss of time, necessitated in destroying a very large portion of the cortex, would be too great. I prefer therefore to make a great number of needles work at once. Upon a circular plate, a centimetre in diameter, I fastened 14 English sewing needles at equal distances . . . I have also made a larger one, 2 centimetres in diameter, with 40 needles. If one applies this machine 30 or 40 times

to the same skull, changing the position of the needles each time, one may be sure that the perforated cortex of the brain is thoroughly destroyed. The bleeding from the operation is inconsiderable. The healing of the wounds is however unfavourable.

"While thinking over the matter, and how the deficiencies in this method might be supplied, my friend, Professor Kundt, drew my attention to White's boring machine, which was originally intended for the purposes of dentistry. This machine, driven by a treadle-wheel, sets a number of small instruments in very rapid rotatory motion. I employ this apparatus with a small circular saw, which is driven by the wheel. One can transfer the rotating saw to any part one pleases of the exposed brain." (P. 131.)

"It will be interesting to know how an animal behaves, which has lost both quarters of one side, *i.e.*, the cortex of one half, and if it is in any way distinguishable from one which has been operated upon cross-wise. Finally we shall see what peculiarities are manifested by animals, which have been deprived of three quarters, and what difference there is if the quarter still left is before or behind. I have experiences on all these points already, and am very busy in enriching them." (P. 133.)

Professor Goltz saves us the trouble of comparing his dates and finding out the length of time his victims survived. He gives us the history of the whole process briefly enough.

"The dates of the four operations," he says, "were respectively:—

"2nd of July, 1879, left anterior.

"7th of October, 1879, right posterior.

"2nd of December, 1879, left posterior.

"10th of February. 1880, right anterior.

"On the 21st of February, 1881, the dog was killed. He lived a year and eleven days after the last operation." (P. 134.)

Among other "trials," his eyes were both plastered up. "He behaved most angrily, and tried to get rid of the plaster with his fore-paws. But he was so awkward at it, that he failed. Instead of moving about as usual, he scarcely stirred from his place. On the following day he began to wander about, but struck his head perpetually against the wall, and so forcibly that one could hear it some distance off." (P. 136.)

"That he was easily irritated by being roughly handled, was mentioned before. On the other hand, he never showed a sign of pleasurable emotion. In the last months of his life, we never observed that he wagged his tail. We could not make him play, though before the last two operations he was very playful." (P. 138.)

"Play!" Poor blinded, mutilated thing, if his tormentors expected him to play with them, it must have been as the frog joins in the game with the boys who stone it.

Another dog was operated on for the first time on June 2nd, and the second time on October 27th, 1880, and was killed on the 14th of February, 1881. Its brain when examined after death was extraordinarily light; the Professor hereupon remarks "Whether this is an exceptional case, or whether the cortex and the other portions of the brain are knit together by a mysterious connection of nourishment, the future will teach us." (P. 141.)

Will no one interfere to prevent the realization of this hideous prospect? Must more brains be pricked through with plates of forty needles, forty or fifty times applied; and sawn into pieces with circular saws, must more eyes be blinded, more senses blunted, more natural instincts violated, more healthy appetites for innocent pleasure destroyed? Is the future to be darkened as the past has been by the record of these deeds?

Another dog was operated upon on June 2nd, and on October 27th, 1880, and killed on 14th February, 1881.

Another was operated upon on July 30th, and on 17th November, 1880. This last was a terrific operation. Professor Goltz says, "In spite of the frightful depth and extent of the injury, the animal recovered even from this experience. On 7th February, 1881, a similar operation was at last attempted on the right side. When the creature awoke from the chloroform-trance, it went staggering about in its cage, gave no signs of consciousness when called, would take no food, and died during the following night."

The Professor's comment is merely "As the animal survived the last operation so short a time, it was impossible to make sure of the effects; the only thing to be made useful for the future, is the fact that the dog could still move." (P. 148.)

And finally on p. 155, Professor Goltz says, "Here I will add the appearances shown by a little dog, which has survived four operations, and which is still alive."

Still alive ! What is in store for it ? We must not pursue *that* thought. There is no law in Germany which can protect that unfortunate creature ; carefully kept alive from week to week and from month to month,—for further torment.

We could fill pages with further details, but we have said enough. Let us give the conclusion of Professor Goltz, on his last page, as to the practical value of his experiments.

" I myself," he tells us, " do not in any sense claim that my researches can be of any use at present for the pathology of the human brain. Let the pathologists go on collecting facts ; then, the apparent contradiction between the experiments on animals and the observations at the sick-bed will be explained and reconciled." (P. 177.)

So that for all this reckless cruelty and wanton sacrifice of far more than life, no practical utility to man is even claimed by its perpetrator. There is no question of the much talked-of " good of humanity," no question of that hypothetical " great right " the " little wrong " is to achieve. No question of anything but just the gratifying of scientific curiosity—and the glorification of Professor Goltz.

And there is no law yet in Germany—if English scientists could have their way, there would be no law in England—to " curb this cruel devil of his will."

One word more. In the *Contemporary Review* for May, Professor Gerald Yeo assures us, " I should be indeed sorry did anyone imagine that I adopted Miss Cobbe's view of Professor Goltz's character ; for I know him too well, and am proud to call him my friend." There

is a saying that a man is known by his friends. Perhaps it applies also to a physiologist? This book which we have been examining was published in 1881. We, at least, now know Professor Goltz too well to desire any place among those to whom he has dedicated his book—"his friends in England!"

CHAPTER XVII.

FLOURENS, GOLTZ, MUNK, FRITSCH, HITZIG, FERRIER AND OTHERS, VERSUS FERRIER, HITZIG, FRITSCH, MUNK, GOLTZ, FLOURENS AND OTHERS.—JUDGMENT.

EXPERIMENTAL investigations of the Functions of the Brain are, it is hardly necessary to say, amongst the foremost of the examples commonly quoted by vivisectionist advocates as illustrative of the inestimable advantages of the "Advancement of Medicine by Research."

Of what is meant in this instance by the delicately euphemistic term "experimental investigation" the last three chapters will have given a faint—a very faint—idea.

Manet this question of the all-excusing utility and importance of its outcome.

As a strictly practical test of this favourite position let us give, in simplest form, a few—a very few—of the nett results of this method of enquiry as found, not in the voluminous works of the enquirers at large but in the foregoing very brief summary of two of the briefest of them.

Flourens, Fritsch, Hitzig, Munk, Goltz, Nothnagel, Carville, Duret, Soltmann, Ferrier, Nicati, Tamburini,

Luciani, Richet—a pretty fair array of authorities ; to be supplemented if need be by the names of Brown-Séquard, Vulpian and others, of equal weight but, as it happens only cursorily referred to in the two treatises immediately under consideration.

And this is the happy—the overwhelmingly convincing—

RESULT.

FLOURENS—*is contradicted by*—Hitzig, Fritsch, Ferrier, Munk—and others.

RICHET—*is contradicted by*—Goltz.

HITZIG and FRITSCH—*are contradicted by*—Munk, Ferrier, Goltz—and others.

CARVILLE and DURET—*are contradicted by*—Soltmann, Munk and Goltz.

NOTHNAGEL—*is contradicted by*—Schiff, Goltz and Munk.

SOLTSMANN—*is contradicted by*—Munk, Carville, Duret and Goltz.

GOLTZ—*is contradicted by*—Ferrier, Munk, Hitzig, Fritsch—and others.

NICATI—*is contradicted by*—Goltz and Munk.

LUCIANI }
TAMBURINI } *Are contradicted by*—Munk and Goltz.

MUNK—*is contradicted by*—Flourens, Ferrier, Goltz, Hitzig, Fritsch—and others.

EVERYBODY—*is contradicted by*—Goltz.

FERRIER—*is contradicted by*—Everybody except Fritsch and Hitzig, who loudly accuse him of having robbed them.

We decline to gild the virgin gold of this simple analysis by any feeble moral of our own.

CHAPTER XVIII.

LISTERISM.*

BEFORE coming to the details of my research, perhaps I may be permitted to explain certain general principles which are essential to my argument.

The first is, that in the discussion of such a question it is advisable to avoid as much as possible mere empirical statements, and to reduce as much as is practicable all our arguments to something like the tests inflicted by the ordinary canons of evidence. This is no easy matter in our professional details, for it is the exception rather than the rule that we can do more than say that we think a certain proposition to be true. The results of our individual experiences vary so much in extent and in conditions that in almost all our arguments we are driven to the votes of authority. Therefore, when we can proceed upon a purely inductive method the conclusion will probably prove a strong one.

My second proposition is not so secure, but still I attach much importance to it; and it is that the narrower the area over which an experiment is tried, the fewer the elements of disturbance in its conditions and the greater similarity in the conditions of detail,

* Extracted from a paper read before the Surgical Society of Ireland. By LAWSON TAIT, F.R.C.S., Surgeon to the Birmingham and Midland Hospital for Women; Consulting Surgeon to the West Bromwich Hospital, &c.

the surer will be the conclusion. Thus my own practice is almost entirely limited to abdominal surgery; and, therefore, an experiment based upon the application of a particular theory to surgical practice will meet with a trial more free from disturbing elements than if I recorded my experience of a mixed practice of amputations, lithotomies, ovariectomies, &c. The reason of this seems to me clear, in that one ovariectomy is of closer value to another ovariectomy than either can be to an amputation, or a lithotomy; and that a series consisting purely of repetitions are better for this purpose than a mixture.

This proposition seems to me self-evident, but if it is not admitted much that I say must fall to the ground. But, mark, I do not mean to assume that a principle which is found absolutely true for the performance of an ovariectomy must necessarily be as true for an amputation. But the proof that it is not true must be shown clearly; and no statement to that effect must be merely asserted, because so many general principles are found to be absolutely true in all such cases.

My third proposition is an empirical one, and open to controversy; but it has received such general acceptance that I must press for its acceptance. It is to be given as a group of conclusions, the first of which is that in the peritoneum we have a cavity peculiarly liable to what we call septic influences. We all know what we mean by these two words, though they defy explicit definition; and the great fatality attending all abdominal operations until quite lately was universally set down to these "septic influences." I have heard it asserted lately that this is not so, and that really the peritoneum seems to be the only part of the body over which septic poisons possess no

influence at all. This astounding statement arises out of the thoughtlessness of some of Mr. Lister's too enthusiastic disciples, who finding that my experience shows that in abdominal surgery we do better without Mr. Lister's details than with them, turn round and utter this extraordinary and revolutionary contradiction. Yet it is the uniform practice of every surgeon about to perform ovariectomy to remove his patient from all possible and recognised sources of septic poisons. This is more rigidly carried out in this particular line of practice than in any other, so that the performance of ovariectomies in general hospitals has been very widely condemned, and its results have never been good. I assume, then, that the field of abdominal surgery affords an especially favourable opportunity for the trial of a system which is based on antiseptic theories; and on this assumption I made an experimental research on Mr. Lister's methods, the details of which I propose to discuss.

Let me first of all say that the theory of putrefaction upon which Mr. Lister bases his practice I regard as having been long ago proved. No known putrefaction can occur save in matter which is dead, and only by means of the admission to it of resting or swarm spores of some of the low forms of life, such as bacteria, bacilli, &c. The only point between Mr. Lister and myself is that Mr. Lister assumes for living tissue the same series of phenomena as he finds in dead infusions, and this I deny altogether. Neither Mr. Lister, nor any of his numerous followers, have ever tackled this important point, and the one experiment upon which Mr. Lister hangs so much, in fact that single argument of his, which has proved such a crux to everyone, rests on this point.

He has told us over and over again, and there is abundant evidence of the correctness of his assertion, that if a deep wound is filled with blood clot, and is dealt with completely by the antiseptic method, that it will become organised and will ultimately form part of the tissue in which it is placed. Everyone knows that without the application of Mr. Lister's details blood clot will putrefy, break down, and disappear. There can be no doubt whatever that here Mr. Lister has established a point of immense importance practically, which is, however, limited to this, that artificially we can make a blood clot do on the external surface of the body what it nearly always does when sealed up in the tissues or in a cavity. Hundreds and thousands of blood clots are organised in the body when covered up, and we think nothing of it. How is it that they do not become so organised on the external surface when ordinary atmospheric air is admitted to them? The answer admits of no dispute, the putrefactive germs breed in them, destroy them, and break them down by putrefaction. You may say, then, that I concede Mr. Lister's arguments, but my answer is that I am not yet done. I have to satisfy myself that a blood clot is dead, or that it is not dead before I go further, at least this was my former opinion. It is probable that the large clots found in the peritoneum, in cases of hæmatocele, do not die, otherwise we should not expect to see many cases of recovery, whereas death is the exception and not the rule, save when it occurs immediately from the hæmorrhage itself. Subcutaneous ecchymoses do not die, and enormous clots are seen to organise and disappear in the scrotum and vulva as long as the skin protects them entirely. The unbroken tissue in fact protects them from the killing

influences of the germs. It might be assumed, in fact, that a blood clot is really not dead, but that it is only in a condition in which vitality is so feeble that it is very easily destroyed, and the amount of vitality is not such as to enable it to resist the attacks of bacterian germs which speedily kill it. This is perfectly in harmony with Mr. Lister's own views, for amongst the many changes of platform to be found in the history of his theory and practice, is the very frequent admission that living tissues have a varying power of resistance of the attacks of germs. We may assume that coagulated blood, maintained at its normal temperature, has the most feeble kind of resistance, and speedily falls a victim.

I further assume as proved that Mr. Lister's details, when carried out, amply suffice for the destruction of germs, and that you may keep a blood clot indefinitely in a solution or under a spray of carbolic acid. If, therefore, you keep a blood clot, not yet dead, at a normal temperature, and fully protected by the details of Mr. Lister's method from the attacks of bacterian germs, you may give it time to become so fully organised as to acquire a power of resisting those germs equal to that possessed by ordinary tissue.

I admitted all this long ago, and I have often confessed that the story of the antiseptic blood clot was a great crux. But the mystery has been fully solved in a most unexpected direction by Dr. D. J. Hamilton, of Edinburgh, in his remarkable papers on "Sponge-grafting." There we find this unexpected display, that a piece of sponge will do quite as well as a blood clot, and that the antiseptic details are altogether needless. The sponge is dead enough, beyond doubt, and no carbolic acid or other agent is in the least needed to enable it to become

organised, or at least to enable it to be the basis of new structure, for perhaps that is the better way to state the case, and it is clearly also the truer way to speak of the blood clot.

Nothing more amazing, nothing more gratifying to the practical surgeon has been produced in this century than the results of Dr. Hamilton's experiments. That a piece of sponge laid on a wound should become part and parcel of the patient's body is almost incredible, yet it is true, and it seems to me completely to explain the mystery of Mr. Lister's blood clot.

From Dr. Hamilton's conclusions I see that it is not necessary to adopt the assumption I formerly advanced that the blood clot was really not altogether dead, for he starts with the belief "that blood clot or fibrinous lymph plays merely a mechanical and passive part in any situation where it becomes replaced by a fibrous cicatrix, and that this vascularisation is not owing to new formation of blood-vessels, but rather to a displacement and pushing inwards of the blood-vessels of surrounding tissues." Being convinced that the blood clot was just so much dead matter in a tissue, it occurred to him to employ some dead porous animal tissue instead, and for this purpose he selected sponge, and he has proved his case. His cases showed "that even where a wound continues in a putrescent condition organisation will still go on. In the case of the blood clot putrefaction tends to destroy it; in that of the sponge, its texture being more resistant, it does not seem to make much difference." In fact, with the blood clot Lister's details only save the non-resistant matrix from destructive putrefaction till the blood-vessels permeate and remove it, just as they ultimately do the sponge, and Dr. Hamilton's experiments utterly

destroy the last surviving argument of Mr. Lister. He compares the "sponge and the blood clot, and says of the latter that it is an excessively porous substance. It is more finely porous than the finest sponge. The fibrin forms a network containing only a few blood corpuscles, while the serum is squeezed out of, or drains away from, the interstices. It is, therefore, a tissue which, if its assumed vital properties be laid aside, is extremely like a sponge in its structure, each being composed of a delicate framework with large and small meshes." He speaks in the same way of fibrinous lymph, and his marvellous experiments explain our facts and destroy the last support of Mr. Lister's theories.

I have, therefore, got so far as to have formularised my views to the effect that, whilst accepting the germ theory of putrefaction in its entirety, I entirely repudiate Mr. Lister's application of it to surgery. Upon it he bases a certain line of practice, more particularly a method of performing operations, which has received such a wide acceptance, which is spoken of in terms of such enthusiasm by men upon whose statements I can place the utmost reliance, that I am almost tempted sometimes to discredit the evidence of my senses, or to come to the conclusion that the tissues of a patient with a disease requiring a pelvic or abdominal operation act in a manner wholly different from those of patients under all other circumstances.

Putting theory and private conviction aside, and influenced solely by a verdict which seemed almost unanimous, and by the surgical conscience which obliges us all to do everything we can and to use everything we know for the welfare of our patients, I gave Mr. Lister's method a trial, which extended over a series of

abdominal sections. This was composed of nearly a hundred operations, sixty of which were for the removal of ovarian tumours, and the detailed results of most of them and many others are given in the paper I have already alluded to. The conclusions of the figures, which were not, and so far have not been challenged, were in every way against Mr. Lister's practice ; and the influence which has been exercised by this paper has been considerable in modifying the views of a large number of competent authorities upon this important subject.

I announced in that paper that, having come to the conclusion that Mr. Lister's system, when completely used, was prejudicial to my patients, not only in the question of mortality, but in the speed and evenness with which they recovered, I should further inquire into the influence of the method.

For this purpose I divided its details into two groups, those which were essential to the performance of the operation and those involved in the after-treatment of the cases. The latter included Mr. Lister's special forms of dressing, such as protective gauze impregnated with carbolic acid, &c., &c., and these I promptly and at once discontinued, because I had proved in my paper that the patients did better without them. I adopted the absorbent cotton-wool, that is, cotton deprived of its intrinsic oil, and entirely without any so-called disinfectant, and this material I have used ever since. With this dressing I have treated more than two hundred cases of abdominal section, and these have been open to the inspection of many visitors of distinction and numerous professional friends. The verdict is unanimous that no better results could be obtained than are to be seen with my dry dressings. The

patients recover smoothly, without any exacerbations of pulse or temperature curves, and primary union is the uniform result.

I had to deal, after that point, with the details of Mr. Lister's system, which are supposed to protect the patients from the influences of omnipresent germs at the time of the operation, and with these it was necessary to make my experiment with very great care in the interests of the patients.

For the sake of convenience I may place these details in three groups:—

1. The use of the spray, which I looked upon as by far the most important, since it covered the whole time of the operation, was absolutely consistent with the theory of the system, and seemed to me, from that point of view, of infinitely greater importance than all the other details put together.
2. The preparation of sponges, ligatures, instruments, &c., previous to the operation.
3. Details of occurrence during the operation, such as washing out the peritoneal cavity, &c.

As carbolic acid is the substance to which Mr. Lister has consistently adhered throughout the whole of his work, and as it is that of almost universal acceptance, my remarks are to be taken as applying solely to it. I found, previous to this research, that the substance thymol, introduced by Mr. Spencer Wells, was too dangerous to be used, and the results of my research have seemed to me too conclusive to have any need for further experiment.

In order to secure the complete performance of my research I went to very great trouble about the apparatus, especially about the spray producer. I had a very large one constructed, which would produce a

continuous jet of spray six feet long, and having a base of nearly four feet in diameter, and this could be continuously maintained for about three hours. The spray used at first was from a solution of one in twenty, then one in thirty, then one in fifty, then one in eighty, then one in a hundred. I then tried one in a thousand, and after that I went on with a spray consisting of nothing but steam and common tap water, and my patients recovered as satisfactorily without the carbolic spray as with it; in fact, I may say that they recovered better without the carbolic acid, for whilst using that substance in a strong spray, I had several indications of carbolic poisoning, and I very nearly lost one case. This was in the instance of a child upon whom I operated for pelvic abscess under a spray of one in thirty. Within a few hours after the operation her urine became quite black from indican, and loaded with albumen. She became unconscious, and finally had severe convulsions. Within forty-eight hours all these symptoms passed off, and she made a perfect recovery.

At the beginning of my research I had all my instruments completely covered in baths filled with a solution of carbolic acid—one in twenty, my sponges carefully cleansed and similarly covered, my ligatures scalded and soaked in the same solution for many hours before the operation, my hands and arms and those of my assistant carefully washed and rubbed over with the solution, and every preliminary detail most carefully carried out.

Then, as with the spray, I slowly and at intervals reduced the strength of the solution, and finally I went through all the performances entirely without carbolic acid. In the same way with the details which came into use during the performance of the operation.

Thus, I used to sponge out the cavity of the abdomen with a solution of one in twenty, but now I use only tepid water, without carbolic acid at all; and in cases where there is troublesome bleeding from separated adhesions I pack the cavity with sponges—as many as twenty at a time—without any carbolic acid in them; or I wash out the cavity with two or three buckets of warm water poured in from an ewer, utterly regardless of germs in either air or water.

This research occupied nearly two years, and all through that time I was carefully on the watch for either symptoms or results which would arrest me in my experiment, and show me that I was in error, and that I must retrace my steps and re-establish Listerism in my practice. But I found none, and as all the details of my practice, up to the first of November last, have been published, I need not weary you with them now. Suffice it to say, that since the time when I may be said to have abandoned the practice of Listerism, I have performed 107 completed operations for the removal of ovarian tumours; and of these there have been only three deaths, or a mortality of 2·8 per cent., besides a large number of other operations for removal of diseased ovaries and tubes, peritoneal and hepatic hydatids, tumours of the uterus and kidney—these having a mortality quite as satisfactory, and a success which has already attracted a wide attention. . .

The conclusions I make, therefore, from my research are (1) that the germs which produce the putrefactive changes in dead tissue are harmless when admitted to the peritoneal cavity in the operations such as I perform upon it. The fatal cases of ovariectomy which I have seen since I gave up the use of carbolic acid—three in one hundred and seven operations—were due

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International Medical Congress, after the
of Dr. Keith and myself upon the matter of
spray, Mr. Lister is reported to have said that
possibly in ovariectomy the spray is not necessary. If
this is to be accepted as the last utterances of antiseptic
philosophy, I can only regard it as another illustration
of its marvellous mutability.

Not six months ago ovariectomy was quoted as the
chief and greatest illustration of the wonders of Listerism;
but it happens to be the only surgical area upon which a
strict statistical inquiry can be made; and when that is
done, Listerism is found absolutely wanting. If germs
are so potent in the case of a lumbar abscess, why do
they prove so harmless in my cases of suppurating
hæmatocele? If the serous cavity of the knee-joint is
so susceptible to septic influences, how does the peri-
toneum escape? If you cannot remove a cyst of the
back without the spray, how do I manage to do without
it for a cyst of the belly? If Listerism is essential in
removing a piece of dead bone, how is it I can freely
dispense with it in removing a slough from the middle
of the liver.

I cannot pretend to answer these questions, they are

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CHAPTER XIX.

PASTEUR AND HIS "MICROBES."

It is only by something of a stretch of argumentative license that the operation of M. Pasteur can be classed under the head of Vivisection. But so much has been made of them by the advocates of that practice, in the terrible of arguments more strictly *ad rem* that it may not be inappropriate just to call attention to certain sensible utterances of Dr. Jousset de Bellesme, who, in criticising in the *Progrès Medical*, a lecture delivered by M. Chamberland at the Sorbonne on Professor Pasteur's discoveries, expresses himself as follows.

"While recognising that M. Pasteur has had the merit of discovering some facts of the highest interest, we may be permitted to ask ourselves whether the theories he has emitted as to the functions of microbes in disease have not exerted on medicine the most pernicious influence. To maintain, while leaning on induction, that all contagious diseases are due to microbes, is one of those ideas which, by reason of its apparent simplicity, gives an easy satisfaction to those who do not look beyond the words. The result is that many physicians acquire a tendency to adopt this manner of seeing things which purely and simply carries us back to the middle ages. For, in fact, this explanation is a very old one, and reappears at certain epochs, and after having reigned for some time, always finishes by being repulsed. Raspail was the last who, at the commencement of this century, popularized this theory in relation to the itch ;

and now M. Pasteur has gathered up the succession, and again raised the dogma that all contagious diseases are due to the introduction into the economy of what he calls 'microbes.' What are these microbes? They are microscopic beings of whose nature M. Pasteur does not seem to be very certain. At first he took them for animals of the nature of infusoria, but afterwards created this word, 'microbes' in order to designate them, a word at present is used as synonymous, so to say, with the term virus, which pathologists employ to indicate the agent that determines contagious diseases. We now know with certainty that these microbes are of a vegetable nature.

" M. Pasteur's microbes have this advantage over viruses, that they are to be seen by the microscope; but when we confine ourselves simply and purely to facts, we find that in no one of the virulent diseases of man have there ever been found these microbes to which he attributes the cause of all this group of diseases.

" What was particularly interesting in M. Chamberland's lecture was the perfect good faith and great simplicity with which he brought out in relief the facility that M. Pasteur has always shown in devising explanations, and the confidence with which he transforms pure hypotheses into demonstrated truths. Thus, the lecturer, speaking of the movements of vibriones under the eyes of the spectators, declares that in fact they are true eels!

" To take as a basis of operation a truth which is presented as demonstrated, but which is in nowise so, is to sin against the most elementary rules of a good method. M. Pasteur may affirm as he likes that microscopic beings are never to be found in waters of

springs or in the tissues of animals or plants ; but such affirmations will convince no one, because everyone knows that they are far too absolute. It would be much nearer the truth to say that they are to be met with everywhere. It is not to be denied that the epithelium of certain cavities, and especially that of the mouth, contains well-nigh the entire collection of these microbes, which are represented to us as so redoubtable. Will anyone venture to maintain that solutions of continuity never take place in these mucuous membranes, that in this way the door may not often be opened to the invasion of microbes, and that they may not thus have the opportunity of passing from the saliva into the blood ? How is it, then, that we are not every instant poisoning ourselves with our own microbes ? Let physicians, therefore, not figure to themselves that the problem of contagious diseases is resolved, and that nothing more has to be done than to search for the microbe. We have not reached that point. M. Pasteur's explanations are only hasty hypotheses."

So far Dr. Jousset de Bellesme. Let us see now what is said upon this inoculation question by Mr. Lawson Tait in his just published paper recently read before the Philosophical Society of Birmingham.

"The argument is that by their inoculation the zymotics of domestic animals may be stamped out, and the claim is that it is a great advance brought about by vivisection. But on a little examination it seems to me that both argument and claim break completely down. . . . There are some twenty zymotics amongst our domestic animals to be provided against. Are we to have each of them inoculated some ten or twelve different times, each time for a different

disease? The affirmative reply possesses a strong pecuniary interest for a veterinary surgeon, but a practical man will only smile at it.

"But, to go deeper into the question, we find another and a much stronger objection. Such a process as protective inoculation must always be an inefficient and a temporary measure. To take the case of vaccination and small-pox, it is beyond dispute that vaccination protects the individual to a large extent from small-pox, but it does not protect the community—as may be seen from the ravages it is making at the present time in neighbouring towns and counties. The machinery of vaccination never can be so perfect as to stamp out the disease, and it must be regarded purely as a temporary expedient. The real agent for the stamping out of small-pox is the machinery of a system of sanitary police, such as we have here* But the case is still stronger with the lower animals. With them, as with us, civilisation has introduced zymotic poisons, which are absolutely unknown to the wild animal, and the reasons are not far to seek. In my capacity as one of the managers of a large public institution, I had recently to investigate the cause of an endemic of swine plague, and I found a state of matters which had caused at the same time typhoid fever in a human patient.

"Look at the arrangements of an ordinary British farm yard, and then believe that it is a matter of no wonder that rinderpest destroys the cattle, and diphtheria the farmer's children. The animals spend their lives in houses not lighted and not ventilated, or walk about in a mass of seething filth, on one side of which

* *i.e.*, in Birmingham.

stands the farm house, every room reeking with the stench of the cattle yard.

“ When it begins to dawn on the mind of the British public, that all these diseases, both for man and animals, are absolutely preventible by the simple means of securing fresh air, pure water, and abundant light, they will be banished.”

And on the whole perhaps this may be a more satisfactory process both to man and beast. Perhaps this will be discovered by and by to have been one of the triumphs of vivisection !

CHAPTER XX.

AMICUS CURIÆ—MEDICAL.

A SMALL pamphlet of 78 pp., printed "for private circulation" and bearing the name of Surgeon-General Gordon, M.D., C.B., Retired List Army Medical Department and Honorary Physician to the Queen, is valuable from more than one point of view.

In the first place, though we are very far from applying to this highly interesting work of Dr. Gordon the invidious phrase *fas est ab hoste doceri*, we note, and with extreme satisfaction, that whatever bias there was in his mind when first directed towards the investigation of which the present work is the outcome, was a bias distinctly in favour of Vivisection.

"Throughout a long and somewhat active professional career," he tell us, "I accepted, in the spirit of simple unquestioning faith, the statements boldly made by acknowledged authorities and leaders in regard to such matters, that whatever advance had taken place in the art and science of medicine was due to experiments performed on animals. I was indeed aware that there did exist a class of people opposed to such experiments; also, that they were called by hard names, and that strange epithets were applied to them by those whom, for many years, I had looked upon as constituting the 'orthodox' party. But never did I examine the grounds upon which objections against experiments were based; I took the entire question for granted, exactly as it was presented—that is, from one side."

The International Medical Congress of 1881 however, afforded an opportunity for considering this question, and many others connected with medicine, in a fuller manner than up till then had been practicable, and for several months afterwards, as Dr. Gordon too justly reminds us, the professional journals were indeed "replete with reports and abstracts of papers on, and of discussions which had taken place in, the several sections of that great and important gathering of medical talent."

Then it was, he tells us, that his attention was drawn to the nature of arguments used by the several advocates of "the experimental method of research on animals," in relation to medicine; and his interest was aroused by the fact that between those arguments themselves there existed discrepancies, disagreements, and in some instances absolute contradictions in regard to reported results of even similar experiments. "It seemed to me also," he proceeds, "to be the case that when the attempt was made to sum up the total of the actual and undisputed results obtained by the whole series of 'experiments' detailed, those results amounted to—nothing whatever beyond what was already known. My attention having thus been drawn to the general subject of experiments, I was led to extend my comparative study back from, and, in directions beyond, the Congress. In a word, I took up the whole subject of 'Vivisection'—including not only the literature of the party favourable to, but also that of the party opposed to the practises embraced, for the sake of brevity, under that term. In this manner, the materials for the following pages have been gathered."

And, on the whole, it would be difficult to suggest

a method of consideration, better calculated to lead to a trustworthy result.

In the second place no one will be disposed to question the fitness of Dr. Gordon to deal with this subject from the practical and professional point of view. Retirement from the Army Medical Service after a long career in India, with the distinctions of C.B. and Honorary Physician to the Queen, is not arrived at without some share of practical experience or some capacity for practical work.

Finally, Dr. Gordon's motives are as much beyond question as his qualifications. Professor Owen himself would find it difficult in his case to supplement the lack of argumentative resource by any appeal to that fine vituperative power which, in him, "age cannot wither, nor custom stale."

This much premised, let us, as briefly and rapidly as possible, follow Dr. Gordon in his attempt to collect and arrange in the present paper as many statements of a definite character as are to be met with in all available writings on the several subjects of drugs and poisons in relation to their effects on animals and man. The task, he tells us a little drily, has by no means been as easy as it might at first sight have been supposed to be, "for this reason, that although in the literature of the subject, abstract statements are abundant, those of a precise and definite kind are comparatively few." But he has performed it gallantly and has succeeded in bringing together a mass of evidence the study of which we can heartily recom-

mend to all who care to approach the subject from any other side than that of a determinedly foregone conclusion. For the general reader indeed it might perhaps be wished that a little more attention had been bestowed upon arrangement ; a point in which the work as it at present stands is somewhat deficient. But the labour of collecting the mass of evidence must already have been very considerable and we must not ask too much.

Dr. Gordon begins by summing up the utterances of the vivisectionist party in the profession, and of its principal organs.

"It was asserted," he tells us only too truly, "that with regard to therapeutics as to other branches of medical knowledge, the statements have been made that 'as the beating of the heart and the circulation are necessary for the higher living organisms, so experiments on lower animals might be considered as a condition of life for the science of medicaments,' that 'the advancement of therapeutics was impossible without experiments on the lower animals;' that 'if experiments on the lower animals were prohibited, a number of our fellow creatures would suffer and die for every single life that might be saved among the humbler races.' And so on.

"Briefly rendered," he proceeds, "the following are the points presented in the remarks thus collated, namely :—

"Pharmacology is an experimental science.

"Advancement in it can only be obtained by means of experiment.

"Old theories were discarded as 'observation' was more practiced.

"Yet from this method, in its turn 'erroneous doctrines' resulted.

"Even now the normal functions, and abnormal conditions of the body are not 'thoroughly understood.'

"Some old remedies whose 'physiological action was entirely unknown, are still considered valuable means of treating disease.' "

This is the position he proceeds to test.

One of the most important of the divisions into which the work naturally falls is the question of transferring to the treatment of the human subject the conclusions arrived at from experiments with drugs upon the lower animals. And on this point Dr. Gordon gives a large amount of highly-instructive information. His facts indeed are scattered pretty much through the entire volume, and hence to some extent fail of their full force. But even so they are striking, and collected into a solid phalanx they become irresistible. For instance—

"A few grains of tartar emetic cause almost immediate vomiting in dogs, whereas the same drug, even when given in doses of several ounces, has scarcely any physiological effect on horses. Aloes, the most uniform and convenient purgative for horses, is uncertain and irregular in its action on cattle, but purges dogs in doses of nearly a drachm, or eight times as much as is given to a man. Opium, strychnia, and ether also afford good illustrations of the different effects which the same medicine has on different classes of animals. Horses are particularly liable to superpurgation by medicines, and most substances which act as an emetic for men and dogs, produce a sedative effect when given to horses in sufficient doses. Common table salt, used

as a necessary condiment by man, acts as an emetic on dogs. Buttercups, which cows eat unharmed, produce most distressing vomiting when eaten by children. . . .

"Large doses of antimony may be given to dogs, and little effect be produced, provided that free vomiting occurs, while in some persons quarter-grain doses produce the most utter prostration. The injection of a small quantity of Ferric-chloride has proved fatal in the human subject, yet Grelin found that two drachms produced only vomiting in dogs, and that twenty grains injected into the veins produced no effect whatever. A rabbit can tolerate enormous doses of atropia, administered either by the stomach or subcutaneously, whereas in the human subject its mere application to the skin has caused death in two hours." (Pp. 19-20.)

"Small doses of opium excite tetanus in frogs; on the other hand, birds, namely, ducks, chickens, and pigeons cannot be poisoned by crude opium, and morphia salts must be given in enormous doses. Certain animals, like pigeons and rabbits, appear to be almost insusceptible to the influence of belladonna. It has very little effect on horses and donkeys. Calabar bean destroys birds most easily, while frogs require as much as will kill a dog. Carnivora and herbivora are very differently effected by the tobacco alkaloid. Of conia—Verigo says, 'that in frogs we get paralysis, but no convulsions; but that in animals convulsions occur after large doses, and paralysis of the extremities only after small ones.' Tidy and Woodman say, 'that rabbits are far less influenced by ergot of rye than dogs are.' This they attribute to the circumstance that rabbits 'are accustomed to a vegetable diet, and

because idiosyncrasy greatly modifies the action of narcotics in herbivorous animals.' The same authors say, 'that death has been caused in the human adult by eating fifteen sumach berries, yet rabbits do not appear to be affected by them.' Dr. Nunnely says, 'the physiological action of digitaline on the heart of frogs would appear to be widely different from its therapeutical action on the dilated and weakened human heart in disease.' " (P. 19-21.)

"Two drachms of opium are required to kill a middle-sized dog (Charret, in *Revue Medicale*, 1827, p. 514), while twenty grains have killed a man, and less would be sufficient. Alcohol acts more powerfully on dogs than on man. Opium although deleterious to man and to dog, produces in each different symptoms from those in the other, although on the whole, the difference is not great. In animals, oxalic acid, besides inflaming the stomach, causes violent convulsions; in man, for the most part, it excites merely prostration. Irritant poisons do not cause vomiting in rabbits or horses, because these animals cannot vomit." (P. 24.)

"Poisoned arrows, similar to those by which Commodore Goodenough was killed in the South Seas,' are tried on two dogs and a rabbit by wounding them freely; but without producing tetanus or any other symptoms.

"Experiments on dogs with poison used for arrows by South Sea Islanders did not result in the death of the animals. Thus 'confusion' has arisen from the experiments related.

"Horses can take large quantities of antimony; dogs, mercury; goats, tobacco; mice, hemlock; and rabbits, belladonna, with perfect impunity, whilst small quantities of most of these substances would be

sufficient to kill a man? Again the natural aperient of the dog is couch grass; it has not a similar effect upon man, whereas parsley, an innocent herb to the human being, is a deadly poison to parrots. How is it that most, if not all animals can eat food in such a state of putrescence without any ill effects, as would be sufficient to cause symptoms of poisoning in man?" (Pp. 49-50.)

And finally it appears on the authority of Dr. Budd Painter that that strong-stomached personage the hedgehog will prepare himself harmlessly for his black-beetle supper by a gently stimulative luncheon of Spanish-flies. (P. 66.)

Another point on which Dr. Gordon very rightly insists in this connection and which has received somewhat strangely slight attention from either party in the controversy is the nevertheless well-recognised phenomenon of "idiosyncrasy" amongst mankind themselves.

"The circumstance," he tells us, "is a matter of every day observation that the power of assimilating particular articles of food is affected by the personal conditions of individuals, including their habits, occupations, and *idiosyncrasies*. The extent to which this is so is acknowledged in the shape of a familiar proverbial expression having reference to the fact. And with regard to drugs, their effect for good or evil, depends quite as much upon the *peculiarities* of individuals as do those of food. Nor is it possible from any 'experiment'—other than upon the individual immediately and directly concerned, to acquire a knowledge of the existence of such idiosyncrasy, and of the best method of acting in regard to the individual person in whom it is manifested. On these points a recent writer of high

repute has expressed himself in this way: 'It is now apparent that a patient is not merely a subject of interest as the victim of some morbid process, nor even as furnishing an opportunity for individual advancement merely; he is an elaborate and interesting organism possessing certain definite qualities. In fact he is a man. He is a being who possesses the attributes of humanity collectively; together with some variations which form individual peculiarities.' 'These individual characteristics, in certain combinations, are so pronounced as to form what are called idiosyncrasies. Thus one person cannot take milk, while others cannot eat an egg; others cannot take quinine, or can tolerate some forms of iron only. To one few tonics are endurable, while another seems only to be worse for every conceivable form of neurotic. The intolerance of opium and mercury by certain persons is well known. Chloral hydrate, hyoscyamus, and other neurotics are well or ill borne by different individuals in a curious and almost inexplicable manner. In an aged couple—both of whom were subject to attacks of suppressed gout, chloral was simply a poison to the lady, while her husband's praise of it amounted to eulogy.' (P. 21-22.)

"Occasionally, a drug, whose action is, as a rule, uniform on man, in a few instances, departs from this regularity of action, and produces a perfectly different train of symptoms, constituting in the subject of the new action what is called an idiosyncrasy. As an example of this, the action of opium may be cited. It is, as a rule, sedative; but in a very few instances it is found to possess no sedative powers, but acts as a smart aperient."

Nor is this idiosyncratic peculiarity always the same even in those who are the subjects of it.

"Individual conditions affect very materially the effects of drugs. In a state of sickness the effect of some poisons is not seen at all, or else manifests itself with great rapidity, and such poison will occasion death in even very minute quantities. For example, in cases of tenesmus or tetanus, the patient can take such quantities of opium as would kill him were he in an ordinary state of health; so also, in dementia, cholera, hysteria, and delirium tremens, large quantities of opium can be harmlessly taken." (P. 22.)

Or as it is put by Dr. Rutherford—in whose opinion "no one would dare to test the effect of some new remedy on a human being without first of all experimenting with it on animals"—"Experimenters will never be able to say, that in two different individuals, with livers in a state of torpidity from different causes, the same dose of rhubarb or of podophylline will produce the same effect, or even whether in any case they will produce the same marked effects as they do on dogs. The canine diet and digestion are so different from the human, that it is to be expected that medicines acting upon the digestive apparatus will influence dogs differently from man." He has "given doses of elaterium that would have killed a man to some of the carnivora without causing the slightest purging."

The contradictions alike of fact and theory between rival observers of which we have seen such startling instances, in several of the preceding chapters, as gathered from the records of *Research into the Functions of the Brain*, afford, as might be expected, a fertile topic.

"As an illustration," our author tells us (pp. 13-14),

“of the absolute extremes to which opinion with regard to the employment of drugs in actual treatment of disease has recently attained, it will be sufficient to transcribe the expressions on the subject made use of by very eminent men, each in his particular walk. Professor Huxley expressed himself to this effect:— ‘Living matter differs from other matter in degree and not in kind; the microcosm repeats the macrocosm; and one chain of causation connects the nebulous original of suns and planetary systems with the protoplasmic foundation of life and organisation. From this point of view, (he continues,) pathology is the analogue of the theory of perturbation in astronomy.’ If we consider the knowledge positively acquired in this short time of the *modus operandi* of particular medicines, among which he named curari, atropia, physostigma, veratria, casca, strychnia, bromide of potassium, and phosphorus, there can—he thinks—be no ground for doubting that sooner or later the pharmacologist will supply the physician with the means of affecting, in the desired sense, the functions of any physiological element of the body. ‘It will in short’—so he says—‘become possible to introduce into the economy a molecular mechanism, which, like a cunningly contrived torpedo shell finds its way to some particular group of living elements, and cause an explosion among them, leaving the rest untouched.’”

“In his evidence before the Royal Commission, Sir William Gull, on the other hand, is reported as stating that he was not a believer in drugs. (2. 5545.) Whilst Dr. Wilks,” the distinguished President of the Pathological Society and a vivisectionist almost as enthusiastic as Sir William himself, “says:—‘In the first place it must be admitted that the changes which

of uncertainty and confusion in which conclusions drawn from experiments, remain at the present time. Is it reasonable, therefore," asks Dr. Gordon, "to expect that the confusion now existing is at all likely to be unravelled, or to still further increase as a result of more 'experiments?' Analogy," he tells us, "justifies the reply. Certainly, to increase." (P. 73-74.)

"What," he asks again (p. 59), "becomes of the numerous drugs, 'specifics,' and so on, which are continually being bepraised in journals as sovereign remedies for such and such diseases? The proprietor of the first druggist's shop you meet with, will point to his crowded shelves, and say, 'there they are; no one ever asks for them; they are quite forgotten.'"

"Are more precise details in reference to this point demanded? If so, here are a few. According to information obtained from a dispensing chemist in extensive business in the West End of London, the following drugs have more or less completely fallen into disuse within the past ten years, namely chrysophanic acid, chromic acid, actœa racemosa, iodide of ethyl, aspidium felix mas, brieria anthelmentica (or koosoo), tetra-chloride of carbon (as an anæsthetic) bichloride of methylene, castor, erythroxyton coca. According also to the same informant, several of the above had never been used at all by practitioners in the provinces."

And again (p. 46) in reference to a long list of drugs enumerated among those the introduction of which is assigned by Dr. Lauder Brunton to "experimentation" on animals, he solicits our attention to the remarks of a dispensing chemist, engaged in their sale, whose statements, for the sake of convenience, are given

within brackets;—namely, bromide of ammonium (used as a sedative); iodide of cadmium (very little, if at all used); oxalate of cerium (used for sickness during pregnancy); physostigma (used as a tonic); sumbul (antispasmodic); veratrum viride (emetic, requires caution, very little used); acetic ether (used); nitrate of ammonia (used as nitrate of potass); nitrate of amyl (used in asthma, but apparently is no better than stramonium); areca (a vermifuge); hypophosphite of lime (used for pulmonary affections); chloralhydrate (much used); guttapercha (only used for external application); larch bark (a stringent aromatic); phosphorus (stimulant). With regard to this list, it is to be observed that it contains some drugs the properties of which were recognised from time immemorial in the countries to which they severally belong, namely, physostigma, areca, and larch, others that obtained severe condemnation as leading to evils special to each, namely, chloral, and others which have either never obtained credit, or are already being added to the very long list of disused drugs. Of their value while in use—that use being governed by the results of physiological experiment—let the following afford a suggestion.

“Professor Foster,” again Doctor Gordon reminds us (p. 45), “has devoted much time and pains to ascertaining the fact that chloral reduces the temperature of animals experimented upon, yet of seven cases of hydrophobia (in man), treated in the Manchester Royal Infirmary, two only were treated by chloral, and these two cases registered the two highest temperatures with the exception of one case treated with chloroform, the temperature noted in which comes between the two.” (P. 45.)

Finally, Dr. Gordon summarises generally his conclusions as follows:—

“Although as results of ‘experiments’ and ‘observations,’ old theories were discarded, and ‘erroneous doctrines’ have taken their place, the functions of the body are not yet understood; but value is still attached to some old medicines, the precise manner of action of which ‘was entirely unknown.’

“The plea of ‘Good of Man’ is unsupported by the circumstances related. On the contrary, medicine has thereby been directed into false roads, and therapeutics brought into ‘a most unsatisfactory state.’ Men and women of middle and old age die in larger ratios than formerly, and medical men die of diseases most carefully investigated by them. The plea of ‘Good of Man,’ however, is adduced only by one section of ‘experimenters.’ It is set on one side by the great majority, for that of ‘abstract science.’

“The organisation, and individualities being different in animals and in man, and in particular members of each, results of ‘experiments performed on the one are untrustworthy with regard to the other. States of health or disease also modify the effects of drugs in man.’

“The fallacies of conclusions regarding effects of poison on man from experiments on animals are shown. Against the examples given of cases in which aids are obtained from such experiments in connection with medico-legal investigations, others are adduced of an opposite tendency. It is further shown that convictions have been obtained without the performance of such experiments; also that on fallacious conclusions thus obtained, innocent persons narrowly escaped conviction on capital charges.

"The recent discovery of animal alkaloids having toxic properties, appears to introduce a complicating element into chemical processes in connection with vegetable poisons.

"Drugs and poisons affect differently, different kinds of animals, and all kinds more or less differently from man. This is acknowledged by experimenters themselves.

"Certain medicaments, highly lauded when first introduced, have fallen in reputation, as a result of further experience of them.

"The action of drugs varies according to their dose; and according to the state of the person to whom they are administered.

"The theory of antagonism between poisons is not upheld by facts.

"Therefore, and in brief, the facts are placed beyond question, that in as far as direct results are concerned of experiments with drugs and poisons performed on animals, such experiments in the particular cases related were untrustworthy, misleading and fallacious. That as a result of this method of research, the science of therapeutics has been brought to the state of uncertainty and 'confusion,' in which high authorities describe it as now being. That in medico-legal cases, conviction has been obtained in the absence of experiments on animals with suspected matters, and when chemical analysis failed to detect the presence of poison. On the other hand, cases are on record, by high authority, in which as a consequence of undue confidence being placed in experiments on animals, innocent persons narrowly escaped conviction on capital charges. Such experiments being thus shown to be in themselves useless, misleading, confusing,

and actually dangerous in their application to therapeutics, and in certain medico-legal cases, they become in themselves an evil alike in relation to the subjects of medical treatment based upon 'results' thus obtained, and also in regard to the usefulness of that portion of the medical profession, whose system of practice is based thereon."

CHAPTER XXI.

AMICUS CURIÆ—SURGICAL.

WE cannot better follow up the testimony on the medical aspect of vivisection of the Indian C.B. and Hon. Physician to the Queen, than by the evidence of one of the most eminent—and quite the most rising—of living English surgeons upon its practical surgical bearing.

The name of Mr. Lawson Tait is too well known throughout England to need any introduction here, even had we not already so largely availed ourselves in a previous chapter of his admirable paper upon that much-vaunted but already moribund feat of modern scientific surgery known by the name of Listerism. The paper with a brief review of which we propose to close this present series of sketches of some of the more striking Fallacies of Experimental Physiology, was read in May, 1882, before the Philosophical Society of Birmingham, chiefly in answer to a pamphlet in which a Mr. Sampson Gamgee—a distinguished local surgeon who must not be confounded with the Mr. Arthur Gamgee, who, as reporter for the *Lancet* obtained an unenviable notoriety as one of the heroes of the Ferrier Case—had not long before chanted a half scientific, more than half mystic, and altogether eccentric pæan in honour of the noble art and mystery of vivisection.

. In this pamphlet Mr. Sampson Gamgee, as many of our readers will remember, has set forth the

proposition that without experiments on living animals, "scientific surgery could not have been founded, and its present humane and safe practice would have been impossible." This proposition Mr. Gamgee supports by a rather startling array of instances which we may at least presume are the best and strongest he could find. These selected instances Mr. Tait tabulates, and proceeds to discuss them historically in order.

- I. Treatment of injuries of the head, and the theory of Contre-coup.
- II. Amputation of the Hip-joint.
- III. Paracentesis Thoracis.
- IV. Sub-cutaneous Tenotomy.
- V. Treatment of Aneurism, Ligature and Torsion of Arteries.
- VI. Transfusion.
- VII. Abdominal Surgery.
- VIII. Function of periosteum.
- IX. The Ecraseur.
- X. Detection of poison.

According to Mr. Gamgee the Académie de Chirurgie gave out the subject of contre-coup, and its influence in injuries of the head as the subject for a prize competition, the prize being gained in 1778 by a certain M. Saucerotte, the successful essay being "based on literary research, clinical observations, and twenty-one experiments on living dogs." Mr. Gamgee omits, however, Mr. Tait drily observes "to make any estimate of the value of the experiments on the dogs, which seems to me to be absolutely nothing." This indeed is a little point very often forgotten by vivisectionists, as is also such a trifling matter as Mr. Tait goes on to recall to mind, viz., "that the theory of contre-coup had

been completely established for nearly two centuries before, and had been particularly the subject of Paul Ammannus of Leipsic, who wrote a well-known work, 'De resonitu seu contra-fissura cranii,' in 1674, in which trepanning is recommended at the point of contre-coup, as had been practised by Paul Barbette, of Amsterdam, thirteen years before that." And finally, to make the thing quite complete, our author proceeds, "The theory of contre-coup, and the fatal practices arising from it, are happily now buried in oblivion, in spite of Saucerotte's vivisections, and would never again have been alluded to, but for Mr. Gamgee's unfortunate resurrection of them. The modern verdict concerning fractures of the skull is given tersely in Mr. Flint South's words, 'the less done as regards meddling with them the better,' and 'a knowledge of counter fractures is quite uncertain.' In fact nothing could be more unfortunate than the selection of M. Saucerotte's experiments as an illustration of the value of vivisection, for they were performed for a purpose which was long ago recognised as futile, and in support of a practice universally condemned."

M. Saucerotte having thus been placed in the witness-box, Mr. Tait proceeds to cross-examine him on some other points. He anticipated, it seems, many of Ferrier's—that is to say Yeo's*—experiments by more than a hundred years; finding when he trephined the skulls of dogs and injured their brain on the right side, that a certain amount of weakness was induced on the left side, and *vice versa*. "A fact," Mr. Tait adds, "that had been established by pathology long

* Or Fritsch and Hitzig's ?

before." M. Saucerotte's "idea of imitating the injury of contre-coup," our author tells us, "was to pass a knife right through the substance of the brain, till it impinged on the inner surface of the skull opposite the trephine hole, a most absurd experiment, as the contre-coup injures at the opposite surface only, and not necessarily at all the intervening brain substance. Reading his experiments," concludes Mr. Tait, "they seem so like Ferrier's that I fancy if Dr. Ferrier had known of the existence of this essay he would have found little need to repeat its work."

Here, however, we fancy Mr. Tait is mistaken, probably from having been more occupied in the practical work of his profession than in the study of laboratory literature. The one idea with which an experimental physiologist would seem to be invariably fired on reading of an experiment by another physiologist is to set to work forthwith to perform the same experiment—with an entirely opposite result. The peculiarity in Dr. Ferrier's procedure seems to be that his results follow those of his predecessors as closely as his processes. Which seems to irritate them even more than the other process. With results as shown in Chapter XVII.

M. Saucerotte's conclusions concerning treatment of injuries of the head drawn from these valuable experiments, Mr. Tait says, "are not such as would be listened to in modern surgery, and it is certain that if they were ever acted upon they must have had results almost uniformly disastrous."

This, however, is something of a digression. We proceed to the consideration of the second of Mr. Gamgee's triumphs of vivisection, Amputation of the

Hip Joint, and to what our author describes as that gentleman's "astonishing statement" that this operation was only attempted on the human subject after it had been proved safe by vivisection. "The authority," says Mr. Tait, "he has been kind enough to give me for this is a brief sentence in the preface to the ninth volume of the '*Mémoires de l'Académie de Chirurgie*,' written by the Secretary General and published in 1778. But the first hint we get of amputation of the hip-joint is from a German surgeon named Vohler, who was in practice about 1690. It is doubtful if he ever performed it on a living patient, but it is on record that he tried on the dead body. But it was performed by M. la Croix, of Orleans, in 1748, not only on one limb, but on both limbs of the same patient, the first operation being successful, and the second almost so. This was nearly thirty years before the publication of the vivisection of dogs; and there are many other cases of success previous to Mr. Gamgee's alleged origin of the operation, one being by the celebrated Ker of Northampton, in 1773."

Paracentesis Thoracis, or tapping of the chest, comes next in the list of Mr. Gamgee's selected cases, the invention of this procedure being accredited to a Mr. William Hewson, who based a theoretical operation for pneumothorax—or air in the chest-cavity—upon experiments on living dogs and rabbits so long ago as 1769. This ingenious gentleman "made a wound," observes Mr. Tait, "in the side of the chest and admitted air into the pleura, where no air ought to be, and then he operated to get it out again. When such a condition is brought about in man, and no vital organ seriously injured, the patient gets perfectly well without

any operation. I cannot learn that Hewson's operation for the removal of air has ever been performed on man. When pneumothorax occurs from disease it is generally associated with conditions necessarily fatal, for which no operation is advisable. . . . Finally, tapping for the removal of *fluid* in the chest was practised long before Hewson's time, and therefore his research was needless. Hewson really based his proposal on this well-known practice, but in this he was anticipated in the most favourable cases—those of wounds—for Anel, of Amsterdam, published quite the same proposal in 1707, and it has been uniformly condemned by every writer on military surgery since, because the removal of the air merely induces bleeding. Anel devised a syringe for the purpose, which has been revived as the modern aspirator. Had Mr. Gamgee known anything of Dominic Anel he would never have mentioned William Hewson."

Subcutaneous Tenotomy forms the next item in Mr. Gamgee's catalogue. And with regard to this Mr. Tait tells us that he has traced the history of the surgery of tendons, and cannot see the slightest reason to attribute any of the advances in this department to the alleged vivisections of John Hunter. He cannot even find any record of the experiments themselves, beyond the allusions to them by Drewry Ottley and by Palmer.

"The same accident," he goes on to say, "which happened to Hunter in 1767, happened to the first Monro in 1726, and from the latter instance a very marked advance in surgical practice was at once made, and a contrivance invented by Monro himself, for his own case, is still in use and goes by his name. No such

advance was made from Hunter's accident or from his vivisections. In their histories of the progress of orthopædic surgery Little and Adams make no such claim for Hunter. Adams points out clearly, and with justice, that Hunter established the principles on which subcutaneous surgery is now conducted; but these he established from clinical observations, not from experiments upon animals. And in his lecture on 'Ruptured Tendons' (Vol. i., p. 436) Hunter says not one word about his vivisections, or any conclusions he derived from them as to the method of repair of tendons. If he ever made any such experiments he must have placed very little value upon them."

This little omission on Hunter's part is curiously like what we have already noticed of his own estimate of the value of his vivisectional experiences in the chapter on "Hunter and the Stag." Possibly Mr. Owen's necromantic art will obtain for us before long the same sort of evidence it so opportunely produced in that case of the great disc overer's present more enlightened views.

Mr. Tait however is of opinion that if we trace the development of tenotomy we shall find that Hunter's experiments had no influence upon it at all. "They were performed, it is said, in 1767. But the first tenotomy was not performed till 1784, by Lorenz, at Frankfort, and then the conditions were absolutely in defiance of the principles of subcutaneous surgery. It was done by an open wound, and this practice was continued with hardly any modification till far on in this century. In fact, as Adams points out, it is from 1831 that the commencement of scientific tenotomy dates, at the hands of Stromeyer. If this is so, and Adams makes his case out most conclusively (Club-Foot,

1873), how utterly useless Hunter's experiments on dogs must have been, to lie forgotten and unnoticed till unearthed in Mr. Gamgee's pamphlet of 1882, one hundred and fifteen years after they were performed ; or how singularly careless, and inattentive to the teachings of vivisection the medical profession must be, that they should allow this immense discovery to lie neglected from 1767 till 1831 !"

To bring forward such an illustration as this of the value of vivisection is to cast, Mr. Tait considers, a terrible slur at the profession of surgery, a slur which he does not think at all deserved.

With the Treatment of Aneurism we have already dealt at considerable length and Mr. Tait passes it over with a mere remark as to the thoroughness with which it had been refuted before Mr. Gamgee so naïvely reproduced it. But with regard to the Ligature and Torsion of Arteries, our author is in a position to speak with some authority, having been himself engaged in the performance of what were at the time looked upon by the physiological and surgical world as most important and valuable experiments on living animals, and having "found how futile they are, and how uncertain and untrustworthy are their results."

On this point we give Mr. Tait's conclusions simply in his own words :—

"Mr. Gamgee," he says, "quotes Jones's experiments on the arteries of animals as an instance of a valuable contribution to surgical progress by experiments on animals, and I do not think any more complete illustration could be quoted in support of the uselessness of vivisection as a method of scientific

research than that of the history of the physiological and pathological processes to be observed in arteries. If we consider the question from what some would call the purely scientific side, that is apart altogether from any practical bearings it may have for the relief of human sufferings and the cure of human disease, it consists merely of a mass of observations in which each observer contradicts some other. Upon this subject I wrote as follows so long ago as 1865 :—

“ ‘ John Hunter warned surgeons to avoid injuring any of the coats of an artery, and to this effect advised that the ligature should not be drawn so tight as to cut them ; while many of his contemporaries and successors dreaded any injuries so much that they used all sorts of clumsy contrivances to avoid it—such as pads of lint and bits of cork inserted between the arteries and ligature. Again, Travers, in his experiments on ligatures of arteries, demonstrated that Jones was quite wrong when he insisted that it was necessary to divide the inner coats ; and Mr. Dalrymple of Norwich, proved by his experiments that while simple and continued contact of the parietes of a vessel, without the slightest wound of any of the coats was sufficient to produce permanent adhesion and obliteration, yet that division of the internal and middle coats without continued coaptation invariably failed to produce adhesion. Hodgson says that he cannot substantiate Jones’s statement that division of the coats is essential, and strongly supports the opinion that coaptation of the walls, without rupture of any of the coats, will produce occlusion. The theories of Dr. Jones were strongly supported by Professor Thompson, his teacher, but were strongly opposed by Sir Phillip Crampton, who insisted that the division

of the coats not only was unnecessary, but that it frequently defeats its own object.'—(*Medical Times and Gazette*, 1865.)

"I quote this at length to show that fifteen years ago I found authorities differing so much on this scientific question that I thought it advisable to institute a new series of vivisectional experiments to decide it. The experiments performed by myself only added to the confusion, though nobody saw that at the time. What we were working at was to get quit of the ligature altogether, and to secure arteries by a temporary compression of some kind without injuring the coats. Acupressure promised to accomplish this ; but it failed, for reasons I need not enter into here. The desire to get quit of the ligature was due to the fact that after a vessel was tied one end of the ligature was cut off and the other left hanging out of the wound, where it remained for weeks, sometimes for months, and occasionally (as in Lord Nelson's case) for years.

"The amazing thing is that with all the experiments made upon animals nobody ever thought of cutting both ends of the ligature quite short and closing the wound over it. As a matter of fact, from the time of Ambrose Paré to that of Simpson, an interval of over 300 years, we went bungling on with experiments on animals when the whole thing lay clear before us. It was the successful experiments of Baker Brown, and Thomas Keith upon women suffering from ovarian tumours, which showed us that if we use pure silk, cut the ends of the ligature short, and close the wound carefully over them, success will be certain. Yet not content with this, we hear of fresh experiments on animals with carbolised catgut, chromicised catgut, kangaroo tendons,

and other novelties, which speedily die out when applied to human beings.

“ In the case of the arteries, therefore, experimentation on animals has proved to be ‘ science, falsely so called.’ What we have done in this direction is entirely the result of clinical experience, and that only.”

And so we come to Mr. Gamgee’s sixth example, —Transfusion. And here Mr. Gamgee’s historical knowledge seems again somewhat at fault. Transfusion, Mr. Tait finds, was not initiated, as that gentleman imagines, in the second half of the seventeenth century by Dr. Lower, of Oxford, nor was it first proposed as a legitimate surgical operation at all. It was proposed, and in all probability was really practised together with astrology and alchemy by the “scientists” of the sixteenth century “as an attempt to obtain for the wealthy aged a renewal of their lease of life, after the theory and legend of Faustus.” Allusions to this operation are frequent, the first actual account of its performance however being given by André Libavius, Professor of Medicine at Halle (Helmst. 1602), the blood of a young healthy man being transfused by him in 1594, into a decrepit old man. In the early part of the seventeenth century, Mr. Tait tells us it was a good deal discussed from this point of view, forgotten for a while, and then after the Restoration brought forward again as a topic of discussion, a great deal being written about it both in England and on the Continent.

That under such circumstances it should escape the notice of Mr. Samuel Pepys was not to be anticipated for a moment. And Mr. Tait accordingly gives us the following extract from the famous Diary :—

" November 14th, 1666.—Dr. Croone told me, that at the Meeting at Gresham College to-night (which, it seems, they now have every Wednesday again), there was a pretty experiment of the blood of one dog let out (till he died) into the body of another on one side, while all his own run out on the other side. The first died upon the place, and the other is very well, and likely to do well. This did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop, and such like ; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health, for the amending of bad blood by borrowing from a better body.

" 16th.—This noon I met with Mr. Hooke, and he tells me that the dog which was filled with another dog's blood at the College the other day is very well, and like to be so as ever, and doubts not it's being found of great use to men, and so does Dr. Whistler, who dined with us at the Tavern."

Mr. Tait, however, ventures to differ from these great authorities. For our own part we confess to a certain fascination in Mr. Pepys's "pretty wish." We would not dream of any interference with the Most Reverend arteries of His Grace of Canterbury ; or even—though there is temptation in that idea—with the scientific circulation of his lordship of Peterboro'. But if—by consent, of course—a dozen or so of venerable, most illustrious, world-famous physiologists could but have their carotids comfortably refilled from the ascending aortæ of an equal number of men of plain common-sense, who knows what the result might be ? We might even be spared the need of an Anti-Vivisection Act.

With such experiments as were tried, however, Mr. Tait finds that no good result was obtained. "A large

army of experimenters rushed into the field, a fierce controversy took place: but before the eighteenth century dawned the whole thing was discredited and forgotten. Mr. Flint South gives a succinct history of the matter, and tells us that it was revived by the plan of mediate transfusion in the early part of the present century. The former experiments were fruitlessly repeated and others tried. The result is that the operation has a very insecure hold on professional opinion. I have seen it performed seven times without success in a single instance. I have twice been asked to do it, and have declined, and both patients are now alive and well. We hear a great deal of cases in which patients have survived after transfusion has been performed, but we hear little or nothing of its failures."

In Abdominal Surgery, Mr. Tait is again on his own especial ground and we again leave him to speak his own words.

"Mr. Gamgee," he tells us, "alludes to a vivisection experiment made by John Shipton, and published in 1703, as having laid the foundation for the recent advances of abdominal surgery, which are attracting the admiration of the whole professional world, and the instances he quotes date so late as 1880. If Shipton's experiment has been so fertile, why has the crop been delayed for one hundred and seventy seven years?"

"But even here Mr. Gamgee is wrong in his history. The whole progress of abdominal surgery dates from the first successful case of ovariectomy performed by Robert Houston in 1701. Failing to see the lesson taught by this, and led astray by vivisection, no further success was achieved till 1809, by Ephraim McDowell, and it was not till 1867 that any substantial gain was

made. Disregarding all the conclusions of experiment, Baker Brown showed us how to bring our mortality of ovariectomy down to 10 per cent.; and again, in 1876, Keith proved that it might be still further reduced. The methods of this reduction were such as only experience on human patients could indicate; experiments on animals could and did teach nothing, for operations have been performed on thousands of animals every year for centuries, and nothing whatever has been learnt from this wholesale vivisection.

"As soon as Keith's results were established abdominal surgery advanced so rapidly that now, only six years after, there is not a single organ in the abdomen that has not had numerous operations performed upon it successfully. I have had, as is well known, some share in this advance, and I say without hesitation, that I have been led astray again and again by the published results of experiments on animals, and I have had to discard them entirely.

"Speaking of some recent attempts which have been made to operate on cases of cancer of the stomach, Mr. Gamgee says: 'Warranting, as such cases do, the placing of cancer of the stomach amongst diseases curable by knife, do they not also justify the vivisection of dogs by Shipton and Travers, who, by their experiments, laid the first scientific foundation of intra-abdominal surgery?' Such a statement as this must be so completely qualified as to be regarded as altogether inaccurate. No form of cancer is yet known ever to have been cured, either by operation or anything else. If removed it invariably returns, and in all these cases of cancer of the stomach quoted by Mr. Gamgee, save one, the disease speedily returned and killed the patients. The one exception has not

yet been under trial long enough to enable us to give an opinion. Doubtless it will have the same end as the others."

Mr. Gamgee's eighth example is the Function of the Periosteum in the formation of bone, and Mr. Tait enters upon its discussion with the dry remark that "the history of the development of our knowledge of the formation and growth of bone is extremely interesting, because it shows how completely misleading are the conclusions based upon vivisectional experiments, and how perfectly the secrets of Nature may be unravelled by a careful and intelligent examination of her own experiments." "No one," he goes on to say, can look now "at a necrosed bone without seeing how completely the whole story is there written." He invites our attention to this history as an exemplification of the fact that not only are the practical details of surgery independent of vivisection for their development, but that what it is now the fashion to call the more scientific developments of physiological knowledge "are equally possible without its aid, and are often retarded by its misguidance."

Mr. Tait then goes on with the history of the question; the first real observer in this department being as he tells us, Jean Guichard Duverney, born in 1648, "who achieved such distinction that Peyer, in a dedicatory epistle, says to him, "*Sempiterna te (Duverneyum) quondam trophœa manebunt et Regi vestro, Academiæ Urbique gloriosum erit tantum aluisse civem.*" Duverney fully describes the method of growth and ossification of bone, with its dependence upon the periosteum for its nutrition and growth, the only thing wanting to the com-

pleteness of his researches being the microscopical knowledge of modern times. He also, like the rest, indulged in occasional vivisections, not, however, with a view to his observations on the periosteum, but on the medulla, and they led him, says Mr. Tait, "into most erroneous conclusions. He cut through the thigh bone of a living animal, and repeatedly plunged a stilette into the medulla, and the animal gave evidence of great suffering. The marrow, he therefore concluded, received a great number of nerves, which passed through the canals in the bone, but which existed only in his imagination. As long as he kept to his clinical observations and anatomical dissections he reached exact conclusions, but as soon as he entered the arena of vivisection he went all astray."

Mr. Tait then gives a highly interesting account of the various discoveries and operations of François Hunauld (1730), Robert Nesbit (1736), and John Belchier who in the same year in which appeared the work of his Scotch *confrère* published, in the Philosophical Transactions, the celebrated discovery of the property of madder for staining growing bone, when used as food by animals, which fully demonstrated the method of growth of bone from periosteum and many other most valuable discoveries concerning bone and its development.

Between 1739 and 1743 again Henri Louis Duhamel-Dumonceau published eight works on the growth and repair of bone all based to a considerable extent on Belchier's discovery.

Duhamel, Mr. Tait tells us, performed many vivisections, "but it is quite clear from his own descriptions that they were failures and did not help him. He says

himself that his conclusions were based on sections which he made of specimens of fractures which were in the collections of Winslow, Moraud and Hunauld. In fact to any intelligent observer who looks at a preparation of necrosis it is evident that no vivisection was needed to show the whole process and growth of repairs of bone."

Since his time thousands of experiments upon animals are on record, some proving conclusively that the periosteum has nothing whatever to do with the formation of bone, others proving with equal clearness that everything is the work of the periosteum. "It would be really amusing," says Mr. Tait, "to read the accounts of the researches of Sue, Bordenave, Delius, Dethleff, Fongérons, Haller, and countless others, were not the humour of their mutual contradictions sadly marred by the accounts of the tortures they inflicted uselessly on myriads of animals."

From the scientific point of view Mr. Tait prefers the experiments of Dethleff of Göttingen in 1752 to those of Mr. Syme in 1837, quoted by Mr. Gamgee, the conclusions of both being in his opinion equally erroneous. Haller again, the most distinguished physiologist of his day, made numerous vivisectional experiments with the result of arriving at the conclusion that the periosteum has nothing whatever to do with the formation of bone. "The fact is," says Mr. Tait, "that as long as dependence was placed on vivisection, so long did one experimenter investigate after another fruitlessly, and with conclusions absolutely contradictory. On pathological research alone has the true conclusion been established. Haller made a long series of vivisectional experiments, published in two memoirs, and triumphantly proved that the periosteum can have nothing to do

with the formation of bone. He concluded from his vast array of experiments that bone grew from the middle and not from the outside, together with many other absurdities, only to be matched in the modern researches of Bennett and Rutherford on the function of the liver, also based on fallacious vivisections."

After a brief reference to some cruel and futile experiments by Hunter and Stanley, Mr. Tait comes next to Mr. Syme's paper in 1837, "On the power of the periosteum to produce new bone." Now Mr. Syme, he tells us, was almost every week in the habit of cutting through great thicknesses of new bone attached to and growing from the periosteum to get at dead old bone from which the periosteum had been separated; and the new bone, being between the periosteum and the old bone, must of necessity have grown from the periosteum, there being nothing else it could grow from. "Therefore, if Mr. Syme found it necessary to cut up animals to find out what was constantly staring him in the face, he was a profoundly unscientific surgeon, whose researches were as badly conducted as they were useless." And a profoundly unscientific surgeon our author proceeds to show that he was.

"When Mr. Gamgee," he says, "read his paper at the local Medical Society and quoted these experiments of Mr. Syme, I said that as far as I could recollect, the fact was that their conclusions had been absolutely upset by Mr. Goodsir, who did not make experiments upon animals, but followed a far more scientific method of research—microscopic examination. On refreshing my memory I find this is the case. In a paper read before the Royal Society of Edinburgh in answer

to Mr. Syme, Mr. Goodsir shows that Mr. Syme's method of research was so bad that the experiments, could not be performed accurately. Mr. Syme was pre-eminently an unscientific surgeon, for he knew nothing of the microscope; in fact, it may be doubted if he ever looked through one. Mr. Goodsir, on the contrary, may be looked upon as the father of modern histological research. He proves conclusively that Mr. Syme's experiments were absurd in their conception and futile in their application. Mr. Goodsir's conclusions are, on the contrary, uniformly accepted, and as to his method he says that they were made upon shafts of human bones which had died,—museum specimens, just as Duhamel's were. . . . He condemned the employment of vivisection as useless and misleading, and to him we owe the completion of Belchier's and Duhamel's research,—a completion which was hindered for a century by the blunders of vivisectionists."

The Ecraseur, or instrument for amputating limbs, &c., on the principle of the wire loop with which cheesemongers are in the habit of performing their bloodless operations, is Mr. Gamgee's next instance of the influence of vivisection on the progress of human surgery. No more unfortunate instance, Mr. Tait considers, could be quoted. "The principle of the instrument is that it crushes and tears the tissues instead of cutting them as by the knife. The surgical aphorism that 'torn arteries don't bleed' was in existence long before M. Chassaignac was born, and if he had based its employment on that alone he could have done all that his instrument has effected. But unfortunately he performed experiments upon animals, and immediately he was led astray. I once saw the

leg of a favourite dog amputated at the hip joint on account of disease, and when the limb was removed not a single vessel bled, and the main artery was tied only as a matter of precaution. In the human subject I have seen twelve or fifteen arteries tied in the same operation, for with us the smallest arteries bleed and require to be secured. Our arteries act in ways altogether different from those seen in the lower animals. . . . It may be easily imagined, therefore, that M. Chassaignac's application of the ecraseur to the lower animals was found wholly misleading when man was the subject, and now in human surgery its utility is extremely limited; that is, it is entirely confined to operations, where only very small arteries are divided. Speaking for my own practice, I may say that it might be dispensed with and never missed. Mr. Gamgee's quotation of its application to the ovarian arteries of the cow is peculiarly unfortunate, seeing that when it was used for the same purpose in the human subject it had speedily to be given up on account of its failure."

And so we come finally to the Detection of Poison, with regard to which Mr. Tait frankly professes himself prepared to "advocate the performance of a hecatomb rather than that such a scoundrel as Lamson should escape." So lately as a few weeks before the reading of his paper he made, as he tells us, "a reservation on this point in his condemnation of vivisection as a method of research," but, he goes on to add, "it seems to me, from a closer consideration of the facts of the case, that it forms really a very strong argument for the complete abolition of vivisection, and, at the same time, unfortunately it is a matter of grave reproach to modern science."

Mr. Tait considers the conviction of a poisoner to be almost certain. If the murderer be not a doctor he commits the crime so clumsily that he cannot escape. If a doctor, he must have an interest in the victim's death, is almost certain to be in pecuniary difficulties, and is sure to have had a bad character previous to his great crime. The only difficulty, he considers, lies in the proof of the presence of the poison. But it is only with the alkaloids that this difficulty exists; and as the alkaloids are almost exclusively in the hands of chemists and doctors, their use is very strictly limited.

The most notorious case in which an alkaloid was used, or supposed to have been used by a poisoner, was that of Parsons Cook. "The alkaloid," says Mr. Tait, "was supposed to be strychnine, and I say supposed, because I rise from the perusal of that trial with much doubt as to whether Parsons Cook really died of strychnine poisoning. Certainly I cannot accept it as proved, and I think if the trial were to occur now the same evidence which convicted Palmer would probably break down." It is satisfactory, however, for many reasons, to find that Mr. Tait has no doubt but that the accused received substantial justice.

"In Palmer's case," he continues, "the principal witnesses for the prosecution were the late Dr. Alfred Swayne Taylor and the late Sir Robert Christison, certainly the greatest toxicologists of this century. Strychnine was not discovered in the body of Cook, and Dr. Taylor had to admit that the best tests then known were insufficient to discover one-fiftieth of a grain, and that even half a grain might remain undetected amongst food in the stomach. Palmer was sentenced to death upon the 27th of May, 1856, and in July of the same year a method of chemical analysis

was published by Copney in the *Pharmaceutical Journal*, by which one five hundred thousandth of a grain of strychnine could be detected with certainty after separation. In his evidence Dr. Taylor admitted that the experiments he had performed upon animals with strychnine were practically worthless for any application to man, and in the Report of the Royal Commission of 1876 he condemned such experiments, particularly those which are directed towards the discovery of an antidote to snake-bite.

We think most people will agree with Mr. Tait in regarding it as a matter for deep regret that it was not till after the trial and execution of Palmer that the chemistry of strychnine was exhaustively examined, and definite and certain tests for it obtained. At the trial there was a regular competition among the vivisectionists, the prisoner's counsel actually urging as an argument that his witnesses had performed ten times more experiments to prove that there was no strychnine than the witnesses for the prosecution had performed to prove that strychnine had been used at all. "Yet in two months," says Mr. Tait, "chemical processes were devised without the slightest aid from vivisection, which detected half-a-millionth of a grain with certainty.

Now aconitine is another alkaloid which has been known as a weapon in the murderer's hands ever since 1841. Yet Mr. Tait tells us that he has looked in vain for any record of a research for a method which will detect with certainty by chemical analysis, as strychnine can be detected. He thinks it possible that such a method will be shortly published, and desires to point out that this discovery ought in the interest of public safety to have been made long ago, not only with regard to aconitine, but with regard

to many other alkaloids which may be used in the same way, and which cannot be discriminated from aconitine, even by experiments on animals. At present when need arises, we have to go back to the uncertain method of experimenting upon animals. "But this," says Mr. Tait, "is not science, if by that word we are to speak of exact knowledge. The very weakness of this method has led to a serious infraction of the principles of our judicial proceedings, for the Home Secretary announced in the House of Commons that the Government, in a case such as Lamson's, could not allow the proceedings of the medical experts for the prosecution to be watched by other experts on behalf of the defence. This is altogether unfair, for with such an uncertain and inconclusive method as that of experimentation on animals, two men, even if appointed by the Colleges of Physicians and Surgeons, and not by the Treasury, may be mistaken, whereas by chemical or spectroscopic analysis mistakes are extremely unlikely, and the more observers there are the better."

Mr. Tait's conclusion is, that for the purposes of criminal investigations experiments on animals should be entirely prohibited, while an exhaustive research should at once be undertaken at the expense of the State, upon the spectrum and chemical analysis of all substances available for criminal purposes. "There is no known substance," he maintains, "of constant character which has resisted the chemists' effort to identify it when it has been properly investigated."

"If all these alkaloids had been subjected to an exhaustive investigation as strychnine was after Palmer's trial, there would have been no need to revert to vivisection in order to convict Lamson, and I do not

think it would now be contended as necessary for the detection of a poisonous dose of strychnine that experiments on animals should be made. Vivisection in this case is therefore not the weapon of science, but is the refuge of incomplete work."

Mr. Tait's observations upon the much-vaunted discoveries of M. Pasteur we have transferred to their more appropriate place in Chapter XIX. dealing with Pasteur and his Microbes.

We should only weaken by any comment of our own this simple statement of the verdict passed upon the claims of vivisection in the region of surgical science by one of the most eminent practical surgeons of the day.

SEVENTH YEAR.



ANNUAL REPORT

OF THE

Victoria Street Society

FOR THE

Protection of Animals from Vivisection.

JANUARY, 1882.

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REPORT.

THE Committee submit with some satisfaction the Report of the progress and operations of the Society during 1881.

Notwithstanding every effort on the part of the members in charge of the Society's Bill the general work and business in the House of Commons rendered it impossible to force the Bill to a Second Reading. The subject however was kept steadily before minds both of members of Parliament and of the political public, and a constant stream of petitions was daily poured in from all parts of the United Kingdom.

During the recess Sir Eardley Wilmot announced that the pressure of other parliamentary work would render it a relief to him to be released from the charge of the Bill. Mr. R. T. Reid, M.P. for Hereford, was by advice of the President requested to accept it. Sir E. Wilmot, it is hardly needful to say, still gives his warm support to the Bill, on the back of which his name still stands in conjunction with those of Mr. Reid, Mr. S. Morley, and Mr. Firth.

Early in spring the death of Dr. Childs and collapse of the *Anti-Vivisectionist* led the Society to anticipate to some extent the long contemplated establishment of an organ for the more adequate ventilation of the Anti-vivisection question. On 1st May accordingly appeared the first number of THE ZOOPHILIST which has since been published monthly and the influence of which upon the movement has, the Committee ventures to think, been satisfactory. Its maintenance has of course involved considerable expenditure; but while the whole weight of this has fallen exclusively upon the Victoria St. Society the Journal itself has steadily been conducted with a single view to the general interests of the

movement altogether apart from any special interest of the individual Society by which it is alone supported.

Very shortly after the appearance of *THE ZOOPHILIST* a circular was published announcing the resuscitation of the *Anti-Vivisectionist* several numbers of which have since appeared at irregular intervals.

Another important feature of the year was the Annual Meeting held at the house of the Lord Chief Justice of England, the reports of which, widely circulated through *THE ZOOPHILIST* and the press generally evidently impressed the vivisectionist party with a strong sense of the growing importance of the movement and the necessity of active measures on their part to resist it.

Accordingly at the *International Medical Congress* in August the most strenuous exertions were made by the vivisectionist wire-pullers who had cleverly contrived to get its organization into their hands. Almost every section of the Congress was converted during a great part of the sitting into a stump-meeting for the glorification of vivisection; whilst at the same time, as Professor Michael Forster somewhat incautiously admitted at Bow Street, a "self-denying ordinance" was passed prohibiting the performance of any experiments, lest the public mind should be shocked by too open a contrast between the carefully modified statements of the speakers and the real facts of the practice they advocated. To such an extent was this cautious concealment carried that even the papers read before the Physiological Section were for the most part carefully suppressed in the voluminous report of the Congress proceedings.

In the course of the Congress a statue to Harvey was unveiled at Folkestone, and Professor Owen took this opportunity of adding his word to the general clamour of the scientists for unrestricted freedom of animal torture. Taking for his text a passage from one of the speeches delivered at the Society's Annual Meeting, in which vivisection was denounced

as a practice which, "while it pandered to scientific curiosity added nothing to practical knowledge," he claimed boldly for any and every investigator liberty to perform what experiments he pleased, and that not for the elucidation of any specified point, but with the general view of seeing what might come of it. In support of which theory he recounted at considerable length the now famous story of how John Hunter had been—as Mr. Owen maintained—led to the invention of his great improvement in the treatment of aneurism by the unlooked-for result of a vivisection performed with an entirely different object upon a stag.

This open challenge from a man occupying so prominent a position the Society promptly took up, its answer being embodied in a short pamphlet entitled "Hunter and the Stag."

In this pamphlet it is conclusively shown on plain grounds of historial and scientific argument: *First*, that the description given by Mr. Owen of the nature of John Hunter's improvement was entirely inaccurate; the mode of treatment which he supposed to have constituted Hunter's new invention having been in use for seventeen centuries.

Second: that the brilliant discovery which Hunter really did make not only had not been made by vivisection, but, on plain scientific grounds, could not possibly have been so made. *Third*, that John Hunter had himself left behind him a full account of the means by which he was led to his great discovery and among which vivisection finds no place. The pamphlet concluded with an earnest appeal to Professor Owen as a man presumably above the need of the ordinary subterfuges resorted to by smaller minds, either to answer the arguments of the pamphlet, or frankly to admit their correctness. Professor Owen has not felt himself able to do either. His only action in the matter has been to publish a plainly false statement of the arguments of the pamphlet and to abuse its writer as a "hired scribe."

Another outcome of the Congress was what is known as

"The Ferrier Case." Accepting as authentic the official reports of the *Lancet* and *British Medical Journal*, the Society proceeded against Professor Ferrier for having experimented upon living animals without the necessary license. The result of the trial and the means by which were insured Professor Ferrier's escape from the trifling fine of £5 or £10 which would have followed on conviction are by this time well-known.

But though the prosecution itself was a failure its effect upon the movement was in every way most satisfactory. In the first place it demonstrated with remarkable clearness, the entire futility of the present Act and the hopelessness of any proceedings under it. In the second it showed to what previously unimagined lengths the Vivisectionist *clique* would go in support of their favourite practice. In the third it afforded perhaps the most striking evidence ever yet obtained of the moral results of that practice as regards those who follow it. And, finally, it worked up the Vivisectionist advocates to such a pitch as to cause many of them to fling aside the mask of moderation which had hitherto been studiously worn and by which many simple minds had been deceived.

Both the trial itself moreover, and the rage it aroused among the vivisectionists had the highly desirable effect of bringing the subject prominently before the public mind. The commencement of 1881 found vivisection a question of deep interest to a comparatively small circle of earnest minds, while to the majority it was a subject altogether unknown. The close of the same year leaves it the theme of every important Review and almost of every Magazine and Newspaper. It has become now one of the burning questions of the day and while the change necessarily involves an enormous increase of labour and a heavy additional outlay it is still one upon which the Committee cannot but heartily congratulate both itself and the Society.

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Miss E. Farrer	0	10	0
J. F. B. Firth, Esq.	0	10	6
Mrs. J. F. B. Firth	0	10	6
Mrs. Cyril Flower..	1	1	0
Mrs. M. Foster	0	10	0
Miss Fox..	0	5	5
" "	0	5	0
A Friend..	50	0	0
" " (through Miss Spriggs)	0	10	0
" " (Inverness)	4	10	7
" " (per Miss Majendie)	0	10	0
Miss M. Fry	0	10	6
Mrs. GABBETT	0	5	6
Miss Gamble	0	6	6
Miss E. Gape	1	1	0
Mrs. Garrard	1	1	0
Miss Gaslee	0	6	6
Mrs. Gemmer	0	4	0
Dr. Gilbert	0	6	6
Miss M. Gilchrist	0	3	3
" "	0	3	3
Mrs. Honeyman Gillespie	0	3	6
The Lady Mary Gordon	1	1	0
Miss Gordon	2	2	0
" "	2	2	0
Mrs. Gordon	0	10	0
J. Graham, Esq.	10	0	0
Hon. Mrs. Ogilvie Grant	1	0	0
J. A. Green, Esq.	0	10	0
Miss Greenhill	1	5	0
Dr. Gryzanowski	0	6	6
Miss Guthrie	0	6	6
" "	0	13	0
" "	3	3	0
" "	0	6	6

					£	s.	d.
Miss HANNAH HADFIELD	0	10	6
Mrs. Haigh	1	6	0
E. J. Hamilton, Esq.	2	10	0
Miss Hampson	5	0	0
Miss Emma Hampson	1	0	0
Miss Jane Hampson	0	7	0
Mrs. Harris	1	0	0
G. H. W. Harrison	0	6	6
Miss Hennell	0	10	0
Lady Gwendolen Herbert	0	10	0
Arthur H. Heywood, Esq.	0	6	6
Miss Hill	0	3	6
Mrs. W. M. Hobday	0	6	6
Mrs. Hobday	0	6	6
Mrs. Luther Holden	5	0	0
" "	0	6	6
Mrs. Holland	2	2	0
Miss F. Holland	0	10	6
W. H. Hornby, Esq.	0	6	6
Mrs. Horniblow	1	1	0
Miss M. E. Horsfell	0	10	0
Miss Hubbuck	0	10	6
Miss Humfreys (Collecting Card)	2	7	0
Mrs. INGLIS	1	1	0
In Memoriam, A. G.	5	0	0
" " E. E. S.	10	0	0
" " "	10	0	0
Mrs. Isaacson	5	5	0
Hon. Mrs. Ives	7	0	0
Miss JACKSON	2	0	0
Miss E. T. Jackson	1	1	0
Mrs. James	6	1	0
Lady Jenkinson	1	1	0
Miss Jerard	0	7	0
Mrs. C. W. Jones	0	10	0
" "	1	1	0
Miss Jones	6	0	0
Miss Joy	0	6	6
" " "	1	1	0
" " "	1	1	0
Lady KEMBALL	5	0	0
Mrs. Kemble	5	0	0
" "	0	6	6

					£	s.	d.
Mrs. Philip Kemp	1	1	0
Madame Croop Koopmans	5	0	0
Madlle. Koopmans	5	0	0
THE Lady Superintendent and Children of St. Nicholas							
Memorial Schools, Brighton	2	0	0
Rev. Charles Langton	5	0	0
" "	3	0	0
" "	0	6	6
Madame Lembcke..	2	0	0
" "	0	19	6
Mrs. Levett	1	1	0
Mrs. M. A. Livermore	0	6	6
Miss Lloyd	5	0	0
Mrs. Lloyd	0	10	6
" "	0	6	6
Rev. C. P. Longland	1	0	0
J. Lovegrove, Esq.	1	0	0
Mrs. Lovegrove	0	6	6
Miss L. Lumsden..	1	10	0
" "	0	6	6
Mrs. Lyon	1	0	0
MISS H. MACAULEY							
Miss Mackenzie	5	0	0
" "	0	10	0
" "	0	6	6
Miss Maclaine	1	0	0
Miss A. Majendie	1	1	0
Miss Majendie	1	2	0
Lady Malet	2	0	0
Miss Mallet	0	10	0
Madam de Manin	0	6	6
Miss Mansel	1	1	0
" "	0	6	6
Mrs. Marriott	1	1	0
Miss Marriott	12	9	6
" "	4	0	0
" "	1	8	0
" "	1	4	0
" "	1	1	0
Miss E. Marriott	1	1	0
Miss Marston	5	0	0
John Martyn, Esq.	2	0	0
Miss Martyn	0	10	6
" "	0	5	0

					£	s.	d.
Princess Mele-Barese	5	0	0
" "	0	7	0
Mrs. Milner	0	10	0
Mrs. Michell	0	10	6
Miss Mitchell	1	1	0
William Mitchell, Esq.	5	5	0
" "	2	8	6
" "	2	2	0
Madame Mohl	1	0	0
Miss S. S. Monro	0	10	0
" "	0	6	6
Miss Moore	1	0	0
" "	0	10	0
Countess Biandrate Morelli	0	6	6
Mrs. E. Morgan	0	6	6
Mrs. Morgan	0	10	6
Mr. and Mrs. Frank Morrison	50	0	0
Mrs. Frank Morrison	40	0	0
Colonel Morrison	1	6	6
Frederick Moser, Esq.	1	1	0
The Lady Mount Temple	5	0	0
Mrs. Murray Ker	1	0	0
" "	0	6	6
" "	0	6	6
Mrs. Myers	1	1	0
Mrs. NAPIER	1	1	0
Miss Newcome	2	0	0
Professor Newman	0	6	6
Mrs. A. Newman	0	10	0
Mrs. Nichol	5	7	0
" "	0	13	0
Major Noël	1	0	0
Mrs. Norris	10	0	0
" "	3	18	0
COUNTESS OF ORFORD	0	6	6
Mrs. PAGET	5	0	0
" "	1	0	0
" "	2	10	0
Mrs. Payne	1	2	0
" "	1	8	0
Miss Parkinson	0	10	0
Mrs. James Pender	1	0	0
" "	0	6	6
Miss L. Perceval	1	1	0

						£	s.	d.
Miss F. E. Perceval	1	1	0
Mrs. E. Phillips	1	1	0
" "	0	6	6
Mrs. Phillipson	2	0	0
" "	2	0	0
Mrs. Phillott	0	6	6
" "	0	6	6
" "	2	0	0
W. F. Phillpots, Esq., and Mrs. Phillpots	1	0	0
Miss M. Pigott	0	10	0
F. E. Pirkis, Esq. R.N., F.R.G.S.	0	10	6
" "	0	6	6
Mrs. Pirkis	0	10	6
The Right Hon. The Countess of Portsmouth..	8	0	0
Mrs. Potts	10	0	0
Mrs. E. H. Power	0	6	6
" "	1	0	0
Mrs. Price	5	0	0
" "	1	0	0
" "	0	6	6
William Price, Esq.	1	1	0
Miss Alice Probyn	5	0	0
Mrs. S. Prout-Newcombe	1	1	
Mrs. Thos. QUINBY	0	6	2
Mrs. R. R. RATHBONE	4	0	0
Miss Rees	0	6	6
R. T. Reid, Esq., M.P.	5	0	0
J. Reynolds, Esq.	2	0	0
" "	1	0	0
Miss Hester Rich	1	0	0
Richmond, S. P. A. V.	0	6	6
E. Ricketts, Esq.	1	1	0
Miss Ricketts	1	0	0
" "	0	6	6
Miss E. C. Ricketts	0	10	0
Mrs. Roberts	0	3	6
W. S. Rockstro, Esq.	2	2	0
Miss Rockstro	2	2	0
" "	0	6	6
Miss Roget	5	0	0
" "	0	16	6
Miss Ross	6	6	0
Lady de Rothschild	5	0	0

					£	s.	d.
Chas. M. Roupell, Esq.	2	2	0
Miss Ryle	1	1	0
Monsieur Jules SCHOLL	0	6	0
" "	0	6	6
Baroness von Schwartz (Elpis Melena)	0	19	6
Miss Sergisson	1	0	0
Mr. and Mrs. Wm. Shaen	2	2	0
W. Shaen, Esq.	0	6	6
The Right Hon. The Earl of Shaftesbury, K.G.	2	0	0
Mrs. Sheffield	1	1	0
Mrs. Sheldon	1	0	0
The Misses Shepheard	1	1	0
Miss Skerrett	1	1	0
" "	0	10	0
" "	0	10	6
Miss H. C. Skerrett	0	10	0
" "	0	10	6
" "	1	1	0
Henry Slatter, Esq.	1	1	0
Mrs. R. S. Smith	0	6	6
Miss Southall	0	10	0
Malcolm Spence, Esq.	1	0	0
Miss Ellen Spence	0	10	0
" "	0	6	6
Mr. Serjeant Spinks	2	2	0
Mrs. Spinks	5	0	0
Miss Spriggs	0	10	0
Mrs. Struthers	0	6	6
Miss Struthers	1	0	0
Miss Stuart	0	10	0
J. Y. Sturge, Esq.	0	10	0
" "	0	6	6
Mrs. Sutton	1	0	0
Miss Anna Swanwick	1	1	0
Mrs. TATHAM	0	10	0
Miss Tatham	0	10	0
Miss G. Tatham	0	10	0
Miss Helen Taylor..	1	0	0
Mrs. T. H. Taylor..	0	10	0
Miss May Taylor (Collecting Card)	0	14	0
P. A. Taylor, Esq., M.P.	5	0	0
" "	5	0	0
P. A. Taylor, Esq., and Mrs. Taylor..	4	4	0
William Tebb, Esq.	5	0	0

					£	s.	d.
William Tebb, Esq.	0	13	0
Herr van Manen Thesingh	34	3	4
Madame van Manen Thesingh	14	5	1
" " "	5	0	0
Mrs. and the Misses Thomas	2	2	0
Mrs. B. Thomas	0	13	0
Mrs. Emma Thompson	3	0	0
Chas. H. Torr, Esq.	1	1	0
Miss Tyrrell	1	10	0
Mrs. VEAUX	1	0	0
Miss Venning	1	1	0
Mrs. WADE	1	0	0
A. de Noé Walker, Esq., M.D.	1	6	6
Geo. J. Walker, Esq.	0	6	6
Mrs. Chas. Walpole	0	10	6
Wm. Dyer-Ware, Esq.	0	6	6
Mrs. Weare	1	1	0
Miss Wedgwood	5	0	0
" " "	12	0	0
" " "	2	0	0
Miss Wemyss	2	0	0
Miss Wentworth	10	0	0
" " "	5	0	0
Mrs. J. West	0	5	0
Mrs. White	2	5	6
Mrs. Margaret Whitfield	0	10	6
Miss Whitehead	0	13	0
" " "	0	10	0
Mrs. Whitwell	1	1	0
Mrs. B. Wilberforce	4	14	0
S. J. Wilkinson, Esq.	0	5	0
Miss Williams	1	1	0
S. D. Williams, Esq.	5	0	0
C. H. Williamson, Esq.	0	10	6
Sir. J. E. Eardley Wilmot	1	0	0
Miss Winstanley	2	0	0
Mrs. Wiseman	0	6	6
Rev. David Wright	2	0	0
" " "	1	12	6
Mrs. R. YATES	0	6	6

N.B.—The above List includes only those Subscriptions received before the close of the year to which it refers. Subsequent receipts will appear in the List for 1882.

THE monthly publication of THE ZOOPHILIST having rendered superfluous the preparation of an Annual Volume of the Transactions of the Victoria Street Society, it is proposed henceforward to publish each Year a simple record of the Dates of the chief events of the preceding year touching the Vivisection question. The following Table will be found to afford a complete Chronology of the Anti-Vivisection movement from its commencement to the close of 1881:—

D a t e s .

1873	
Nov.	Professor Schiff's cruelties discussed at an afternoon reception at villa on Bellosguardo. Memorial urging moderation, drawn up by Miss Cobbe; 700 signatures, headed by that of Mrs. Somerville.
Dec.	Memorial presented. Treated with contempt by Schiff in the <i>Nazione</i> , as emanating from "un tas de marquis."
29	Challenge by Schiff in <i>Nazione</i> to <i>Daily News</i> Correspondent at Florence to come forward and prove facts mentioned in letter.
30	The Correspondent sent to office of <i>Nazione</i> her name and address, also testimony to facts signed by eye-witness (Dr. Appleton, of Harvard). <i>Nazione</i> refused to publish the same, even as paid advertisement.
	[Agitation in Florence taken up subsequently by Countess Baldelli and maintained till the retreat of Schiff to Geneva in 1877. Florentine <i>Società Protettrice</i> founded January 15, 1873.]
1874	
Aug.	Meeting of Medical Association at Norwich. Experiments of M. Magnan and others.
Oct.	Prosecution of Norwich experimenters by Royal Society for Prevention of Cruelty to Animals.
Nov.	Organized Movement against Vivisection commenced in England by Memorial to Committee of R.S.P.C.A. urging immediate efforts for legal restriction of Vivisection, drawn up and circulated by Miss Cobbe and friends.

- 1874**
Dec. Pamphlets, *Need of a Bill and Reasons for Interference*, written and issued by Miss Cobbe.
- 1875**
Jan. 25 Above Memorial bearing 600 influential signatures presented to Lord Harrowby and Committee of R.S.P.C.A. in Jermyn Street, by a deputation introduced by John Locke, Esq., M.P. Committee of R.S.P.C.A. promised to appoint Sub-committee to take action as soon as possible.
 First Sub-committee of R.S.P.C.A., Miss Cobbe (in attendance for the occasion) asked by Chairman, "could she not undertake to get a Bill into Parliament for the restriction of Vivisection?" [First action of this Sub-committee was to send the Secretary, Mr. Colam, to the leading physiologists in London to make enquiries concerning their own proceedings. Beyond this step nothing known till the sitting of Royal Commission, when Mr. Colam presented valuable collection of evidence.]
- Feb. 2** Dr. Hoggan's Letter published in *Morning Post*.
- Mar.** Mr. Jesse's Society for the Abolition of Vivisection founded about this time.
- May 4** Bill for "Regulating the Practice of Vivisection" (prepared at Miss Cobbe's request by Sir Frederick Elliot, revised by Lord Coleridge and others) introduced under sanction of Government, into House of Lords by Lord Henniker.
- 12** Bill "To prevent Abuse in Experiments on Animals," introduced into House of Commons by Dr. Lyon Playfair.
- 25** Lady Burdett-Coutts' letter in *Echo* condemning Lord Henniker's Bill as only demanding restriction of Vivisection and not total prohibition.
- June 22** Royal Commission on Vivisection issued.
- Nov.** Dr. George Hoggan and Miss Cobbe resolved to found a Society "to obtain the utmost possible protection for animals liable to Vivisection." The Earl of Shaftesbury and Archbishop of York first members of the Society. Dr. Hoggan and Miss Cobbe Honorary Secretaries.
- Dec. 1** Article in *Animal World*, page 180, containing following passages: "The Royal Society for Prevention of Cruelty to Animals is not so entirely unanimous as to desire the passing of any special legislative enactment on this subject" (Vivisection) "It has been considered desirable, however, to prepare a Bill which is the exponent

- 1875**
Dec. 1 of the views of the Society." The Bill so described was laid before the Royal Commission (page 336), but neither it, nor any other Bill dealing with Vivisection, has yet been introduced into Parliament by the R.S.P.C.A.
- 2** First meeting of the Committee of the SOCIETY FOR PROTECTION OF ANIMALS LIABLE TO VIVISECTION (afterwards VICTORIA STREET SOCIETY), at 13, Granville Place, Rt. Hon. James Stansfeld in the chair; present, Sir F. Elliot, Miss Lloyd, Mrs. Hoggan, Mr. Shaen, Mrs. Wedgewood, Dr. Hoggan, and Miss Cobbe.
- 1876**
Jan. 8 Report of Royal Commission dated.
Feb. 18 Third meeting of Committee of Society for Protection of Animals from Vivisection, Lord Shaftesbury in the chair.
- Mar. 1** Fourth meeting of Committee of Society for protection of Animals from Vivisection, the Archbishop of York in the chair. The "Statement" of the Society discussed and adopted. The offices, 1, Victoria Street, taken for the Society.
- 2** First meeting of Committee of IRISH ANTI-VIVISECTION SOCIETY. Honorary Secretary, Miss A. M. Swifte.
- 20** Deputation from Society for Protection of Animals liable to Vivisection (henceforth VICTORIA STREET SOCIETY) to Home Office to urge on Mr. Cross introduction by Government of a Bill in accordance with the recommendations of the Royal Commission. Introduced by Lord Shaftesbury, included Cardinal Manning, Earl of Minto, Sir F. Elliot, Rt. Hon. W. Cowper-Temple, Col. Evelyn Wood, Mr. Mundella, Mr. Froude, Mr. Leslie Stephen, &c.
- SCOTTISH SOCIETY** founded about this time.
- 23** Meeting of Committee of Victoria Street Society. Suggestions prepared for the Home Office (subsequently embodied in Lord Carnarvon's Bill in its first form.)
- May 22** Second Reading of Lord Carnarvon's Bill in House of Lords. Supported by speeches of Lord Shaftesbury and Lord Coleridge.
- 23** Articles in *Times* and *Daily News* warmly approving of Lord Carnarvon's Bill. Conference of R.S.P.C.A. and Branch Societies in Jermyn Street. Lord Carnarvon's Bill approved, with some exceptions.
- June 1** First General Meeting of Victoria Street Society at Westminster Palace Hotel in support of Lord Car-

1876

June 1 Carnarvon's Bill. Lord Shaftesbury in the chair. Speakers, Cardinal Manning, Marquis of Bute, Lord Glasgow, &c.

10 The LONDON ANTI-VIVISECTION SOCIETY, inaugurated at a numerously attended meeting at 46, Prince's Gate. The Rev. G. Weldon in the chair. (Offices, 180, Brompton Road).

June 21 The INTERNATIONAL ASSOCIATION for the Total Suppression of Vivisection, inaugurated at a large meeting in Willis's Rooms. Thomas Allen, Esq., in the chair. (Office, 25, Cockspur Street.)

July 10 Great Medical Deputation to Home Office against Lord Carnarvon's Bill.

21 Deputation of International Association to Home Office.

Aug. 10 Lord Carnarvon's Bill—(essentially altered in accordance with wishes of Medical Memorialists)—introduced by Mr. (Sir R.) Cross into House of Commons and read a second time.

15 Bill received Royal Signature, and became Act 39 and 40 Vict., c. 77.

Oct. 18 Committee of Victoria Street Society placed on Minutes letter of Miss Cobbe, intimating that she could only retain office of Hon. Secretary should the Committee see fit to adopt the principle of total abolition, or at least of more uncompromising hostility to Vivisection. Circular issued calling on all members to vote for alternatives of policy.

Nov. 22 Resolution of Committee of Victoria Street Society to carry on Society "for purpose of watching operation of existing Act, with a view to the enforcement of its restrictions and its extension to the total prohibition of painful experiments on Animals."

1877

Feb. 7 Committee of Victoria Street Society resolved to support Mr. Holt's Bill, and issued illustrated placards and handbills over London for such purpose.

Aprl. 27 Second General Meeting of Victoria Street Society at Westminster Palace Hotel, in support of Mr. Holt's Bill. President in the chair. Speakers, Bishop of Winchester, Lord Mount-Temple, Prince Lucien Bonaparte, Prof. Sheldon Amos, Cardinal Manning, &c.

May 2 Debate on Second Reading of Mr. Holt's Bill. Ayes, 83; Noes, 222. Majority against Bill, 139.

1878

June 21 First Annual Meeting of International Association.

1878

- Feb. 23 Meeting of Victoria Street Society at house of Lord Shaftesbury, 24, Grosvenor Square.
- Mar. 19 A. J. Mundella, Esq., M.P., at request of Victoria Street Society, asked for Return of Licenses under Vivisection Act.
- Aprl. 10 First Reading of Baron Weber's Paper, *The Torture Chamber of Science*, before the Dresden Society P.C.A.
- May 16 Annual Meeting of London Anti-Vivisection Society at Willis's Rooms. Sir Thomas Dyer, Bart., in the chair.
- June 20 Annual Meeting of International Association at Willis's Rooms. Rev. Canon Baynes in the chair.
- July 10 Return of Licences (asked for in March) issued.
Day fixed for Second Reading of Mr. Holt's Bill.
Postponed to 23rd, and dropped.
- 23 Paris Congress of Sociétés Protectrices opened. Dr. Hoggan, Delegate of Victoria Street Society.
- Aug. 7 Meeting of Committee of Victoria Street Society. Resolution moved by Miss Cobbe, in compliance with the advice of the President, seconded by Gen. Mackenzie, and carried: "That the Committee will henceforth appeal to public opinion in favour of the Total Abolition of Vivisection." (Dr. and Mrs. Hoggan abstained from voting, and subsequently left the Society with assurances of friendly sympathy.) Title of Society changed to SOCIETY FOR PROTECTION OF ANIMALS FROM VIVISECTION.
- Oct. 16 Southampton Meeting of Victoria Street Society.
- Nov. 12 Opening of Brighton Bazaar for International Association and London Anti-Vivisection Society; £1,250 cleared.

1879

- June 17 Annual Meeting of London Anti-Vivisection Society at Memorial Hall, City. Sir J. Naesmith, Bart., in the chair.
- 24 Third Annual Meeting of International Association at Willis's Rooms. Sir J. Eardley Wilmot, Bart., in the chair.
- July 11 Conference between Victoria Street Committee and International Association in view of fusion. Project postponed.
- 15 Order for Second Reading of Lord Truro's Bill in House of Lords.—Lord Shaftesbury's second speech on Vivisection. Contents, 16; Non-contents, 97. Majority against Bill, 81. Lord Aberdare (as reported in *Standard*, July 16) said at this debate, "That he was President of the Royal Society for

- 1879**
- July 15 Prevention of Cruelty to Animals, and that Society had never advocated the total abolition of Vivisection. He would vote against the Bill."
- Aug. 17 Congress of Societies for Protection of Animals at
18, 19 Gotha. William Gilbert, Esq., and Charles Cobbe, Esq., D.L., Delegates of the Victoria Street Society.
- 20 Foundation of Great German LEAGUE AGAINST SCIENTIFIC ANIMAL TORTURE. President, Baron Ernst v. Weber.
- Nov. 24 Addresses sent by Victoria Street Society to Minister of Grace and Justice in Italy, and to the Syndics of Genoa, Florence, Turin, Padua, Pisa, Siena, Rome, and Naples, praying them to discountenance Vivisection and refuse stray dogs to Vivisectionists.—The addresses were signed by Lord Shaftesbury, the Lord Chief Justice, the Bishop of Winchester, Prince Lucien Bonaparte, Lord Mount-Temple, and Robert Browning, Esq.
- 1880**
- Mar. 16 Day fixed for Second Reading of Mr. Holt's Bill. Stopped by notice of dissolution. [Mr. Holt subsequently declined to seek re-election and Sir Eardley Wilmot accepted charge of Bill.]
- May 16 Annual Meeting of London Anti-Vivisection Society at Willis's Rooms. Admiral Sir W. King-Hall in the chair.
- June 3 Meeting of Victoria Street Society at 8, Cromwell Houses. The President in the chair.
- 17 Meeting of International Association at Willis's Rooms. Sir A. Malet in the chair.
- July 1 Brussels Congress of Societies for Prevention of Cruelty opened. Dr. Hoggan attended as a Vice-President of Paris Congress, aided by grant from Victoria Street Society.
- 20 Memorial to Mr. Gladstone prepared by Hon. Sec. of Victoria Street Society. Signed by 100 Peers, M.P.'s, Judges, General Officers, Heads of Colleges, and eminent authors, and by the Representatives of the International, London, Scottish, and Irish Societies. Presented to the Premier by Lord Shaftesbury and favourably received.
- Nov. 3 New Bill adopted by Committee of Victoria Street Society, and charge of it taken by Sir Eardley Wilmot, Bart., M.P.
- Dec. 10 Important Deputation of Scottish Society to the Lord Advocate of Scotland.
- 16 RICHMOND AND TWICKENHAM AUXILIARY OF VICTORIA STREET SOCIETY founded.

- 1881**
- Feb. 1 Annual Meeting of Scottish Society.
- 3 Notice of new Bill given in House of Commons by J. F. B. Firth, Esq., M.P.
- 10 TORQUAY AUXILIARY OF VICTORIA STREET SOCIETY founded.
- Mar. 23 Day fixed for Second Reading Sir E. Wilmot's Bill postponed to May 11.
- May 1 First number of ZOOPHILIST issued.
- 2 Annual Meeting of London Anti-Vivisection Society.
- 11 Second Reading of Bill postponed to July 13.
- 28 Memorial presented by Lord Shaftesbury to the King of Sweden praying him to consider favourably the resolution of his Parliament against Vivisection.
- 30 EAST LONDON BRANCH OF VICTORIA STREET SOCIETY founded.
- June 25 Annual Meeting of the Victoria Street Society at the house of the Lord Chief Justice. The President in the chair. Speakers: Cardinal Manning, R. S. Reid, Esq., M.P., Sir E. Wilmot, R. H. Hut-ton, Esq., the Lord Chief Justice, &c.
- July 13 Second Reading of Bill postponed to July 27.
- 27 Bill withdrawn; the day taken by Government.
- Aug. 1 WESTON-SUPER-MARE BRANCH OF VICTORIA STREET SOCIETY founded.
- 5 Meeting of the Scottish Society in Edinburgh. Miss Cobbe read address.
- 9 International Medical Congress in London. Unani-mous resolution of members in favour of removal of restrictions on Vivisection.
- Oct. Charge of Bill for total prohibition of Vivisection accepted by R. Reid, Esq., M.P., on resignation of Sir E. Wilmot.
- Nov. 17 Summons heard against Prof. Ferrier, in Bow Street Police Court, prosecuted by the Victoria Street Society for infraction of Vivisection Act. Case dismissed, witnesses swearing that the experiments attributed to Prof. Ferrier by *Lancet* and *British Medical Journal*, had been performed by Prof. Yeo.
- FOLKESTONE BRANCH OF VICTORIA STREET SOCIETY founded.

Victoria Street Society

FOR THE

PROTECTION of ANIMALS from VIVISECTION.

President.—*THE EARL OF SHAFTESBURY, K.G.*

CENTRAL EXECUTIVE COMMITTEE.

The Lady ABINGER.	Lady KEMBALL.
Mrs. ADLAM.	Miss LLOYD.
Hon. EVELYN ASHLEY, M.P.	Miss MARSTON (Hon. Sec. London Anti-Vivisection Society.)
The Lady EDITH ASHLEY.	Miss MONRO.
EDWARD BERDOE, Esq., L.R.C.P.	Col. MORRISON (Royal Body Guard).
The Countess of CAMPERDOWN.	Mrs. FRANK MORRISON.
Hon. EMMELINE CANNING.	The Lady MOUNT-TEMPLE.
Rev. W. H. CHANNING.	F. E. PIRKIS, Esq., R.N., F.R.G.S.
Miss FRANCES POWER COBBE.	The Countess of PORTSMOUTH.
Hon. MILDRED COLERIDGE.	ROBERT T. REID, Esq., M.P.
J. F. B. FIRTH, Esq., M.P.	Sir J. E. EARDLEY WILMOT, Bt., M.P.
W. GIMSON GIMSON, Esq., M.D.	
Miss GORDON (of Woodlands).	
Miss ELLEN E. GUTHRIE.	

Hon. Treasurer.—Hon. EVELYN ASHLEY, M.P.

Hon. Secretary.—Miss FRANCES POWER COBBE.

Secretary.—CHARLES ADAMS, Esq.

To whom Subscriptions and Donations—urgently required for carrying on the rapidly increasing work of the Society—should be made payable. Cheques to be crossed Herries, Farquhar & Co., and P.O.O. made payable at the Post Office, Westminster Palace Hotel.

GENERAL HEAD QUARTERS:—

1, VICTORIA STREET, WESTMINSTER, LONDON, S.W.

Branches and Affiliated Societies.

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